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Erratum. The name of David J. King was accidentally omitted as an author of a paper published in the last December number of this journal. The corrected citation of the paper should read: Scott, J. P., Ross, S., Fisher, A. E., and King, D. J. The effects of early enforced weaning on sucking behavior of puppies. *J. Genet. Psychol.,* *1959,* **95**, *261-281.*

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The Journal of Genetic Psychology, 1960, **96**, 3-46.

THE INFANT'S EARLIEST MEMORY OF INOCULATION: A CONTRIBUTION TO PUBLIC HEALTH PROCEDURES*[1]

New York City

DAVID M. LEVY

A. THE PRELIMINARY STUDY

After a series of inoculations given routinely in the health station, a number of infants display a variety of fears. The fears may be quite specific. They may be aroused only by the sight of the needle and syringe during the preparatory stage of inoculation in the customary examining room. They may become "generalized" into a fear of any contact with a physician and any building that resembles a hospital.[2] The fears may be related originally not to the needle, which may not be seen, but to other aspects of the experience; for example, white coats, the most obvious associated visual stimulus. Again such associated fears may remain fairly specific, as in the fear of a doctor in a white coat, or "generalize," into a fear, for example, of anyone in a white coat, involving fear of the barber and by further association, fear of a haircut.

Presumably most of the fears arising from inoculation are transient. By the age of five years, they appear to be markedly reduced in number. We are still missing exact knowledge of the frequency of fears derived from early inoculations at various age levels, including adulthood. Such knowledge would yield among other things an estimate of the number of people who avoid or postpone medical care because of fear of the needle.

The practical value of the knowledge gained about the earliest memories of inoculation should be a prophylactic one. As stated in a preliminary investigation (2): "Most infants take it (the inoculation) well. Some develop

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* Received in the Editorial Office on November 9, 1959, and published immediately by Editorial decision.

[1] The following physicians cooperated in this study: Drs. N. Aaronson, Milton Aisenson, B. Aaronson, D. Vinski, F. Stoll, P. Lustbader, H. Soifer, B. Goodman, E. Kaplan, B. Ginsburg, A. Wein, D. Golann, L. Scheinman, M. Winkler, A. Scacciaferro, B. Kritchman, I. Ross (deceased), M. Lipsett, I. Beris, K. Freund, J. Forrest, E. Neumann, C. Marker (N.Y. City Department of Health).

[2] The word "generalized" is put in quotation marks to avoid the usual inference that it represents a higher order of thinking. It is generalized only in the sense that a sensory stimulus operates in numerous situations once it has been conditioned by a painful experience. Actually a fear of anyone in a white coat occurred as early as eight months in one of our cases. Such fears precede in time the specific memory of the building which houses the health station.

a definite fear of it. Now it would appear that in the study of the reactions of the infant to the practice of immunization, ' e infant's feelings have been left out. The child-health station offers a ready opportunity to investigate prophylactic agents against anxiety as well as disease."

Prevention of inoculation fears arising from present day procedures in the health station requires first of all knowledge of the infant's memory for a special series of painful events. Until the time comes when some method of inoculation will be devised that does not require the insertion of a needle through the skin, such knowledge may be useful. From our preliminary study (2) we inferred that until the age of eight months not one infant in our series gave any evidence of remembering a previous inoculation. The series included 213 infants, age 4 to 12 months at the time they were observed. Each infant was inoculated at the time of observation, and each had had one or more previous inoculations. There were 69 infants age 7 months or younger (number of cases per month varied from 11 to 22); and 144 infants age 8 to 12 months (number of cases per month varied from 15 to 46). According to the criteria used for proof of memory, one of 46 eight-month-olds, 2 of 45 nine-month-olds, one of 23 ten-month-olds, and 4 each of 15 eleven- and twelve-month-olds "remembered." According to these findings the number of infants who "remembered" rose from about one in 50 at 8 months of age, to about one in 25 at 9 and 10 months of age, to about one in 4 at 11 and 12 months of age. In that study all infants were excluded who received inoculations (chiefly penicillin) outside the health station or who had any hospital experience (1).

The criteria of memory were based on observation and inquiry. In the case of an infant who showed apparently no evidence of apprehension or special tension, who smiled and cooed and looked about up to the time the physician prepared needle and syringe, and who then stopped all random behavior, watched the physician's every move, with increasing tautness of facial muscles and finally a loud cry just before the physician made ready to insert the needle, an inference was made that the baby on the basis of previous experience anticipated the painful event.

In the clear-cut case, of the sort described above, the observation alone seemed conclusive. In such cases the mother was asked about variations in her baby's behavior at the health station on that day.

When the baby gave evidence of "disturbed" behavior (crying, or "fretful," or apprehensive, etc.) on entering the examining room and thereafter throughout the session, with a loud cry at the time of preparation, we relied largely on inquiry in order to determine the source of the difficulty. We inferred that a memory of inoculation was to blame when we derived from the mother such evidence as associated fears, cry on the way to the station, on entering the building, at sight of nurse or doctor. Of the 12 infants who were presumed to have remembered inoculations received at the health station, three were so determined from observation alone, the remaining from observation and inquiry.

Reasons for "disturbed behavior" other than inoculations at the station were numerous. They included chiefly injections outside the station, operative procedures, an illness requiring painful manipulation by a physician, a developing fear of strangers or strange places. In a later study of "disturbed behavior" (cry or other signs on entry, regardless of duration) the item was noted seven times in 139 records taken seriatim. Two occurred at the time of the first inoculation. No "proof" of memory of inoculation was found in any one. There was also no such evidence in the seven cases who had also had inoculations outside the station.

B. The Expanded Study

As a result of the preliminary findings, it was decided by the public health officials to expand this study (*Maternal and Child Health,* Dr. Samuel

TABLE *A*
Items of Information on the Form Sheet

I. Injections outside child health station (see instructions).
II. Order of immunization in C.H.S.
III. Physician's initials.
IV. Date of this immunization.
V. Type of immunization (Check. If more than one, check each type.)
VI. While waiting for M.D. did child:
    (a) witness an immunization,
    (b) hear screaming from exam room?
VII. Child's behavior on entering examining room.
VIII. Did child cry:
    (a) before entering examining room,
    (b) on entering examining room?
IX. (a) Did child have physical exam. before his immunization?
    (b) Did child cry during his physical examination?
    (c) Did child stop crying after his physical examination?
X. Did child cry:
    (a) while physician was preparing for immunization?
    (b) when arm was cleansed?
    (c) when arm was pricked?
    (d) immediately after immunization?
    (e) before or during 2nd inoc.?
XI. Mother's reaction (facial expression, etc.).
XII. Mother's handling of baby.
XIII. When did baby first cry in examining room?
XIV. Do you think this child remembered a previous inoculation?

Wishik, then Bureau Director). Form sheets (Table *A*) and instruction sheets were prepared after a number of meetings and a preliminary tryout. They were distributed to 24 physicians in 19 health stations, in Brooklyn, Queens, Bronx, and Manhattan.[3] A visit was made to each station for the purpose of acquainting the physicians, nurses, and clerks concerned, with the nature of the investigation and the method of recording. Occasional visits to stations were made thereafter though not in a systematic way.

The original plans were to have a record of each series of inoculations made by the same physician operating under standard procedures. Standard procedures required a fixed routine in the examining room, the usual interview with the mother, physical examination of infant, then further conversation with the mother, followed by inoculation in the arm, while baby was in the mother's lap.

Physicians were supposed to wear white coats. The entire series of inoculations were to be seven in all, the first five for pertussis (three) and diphtheria (two) starting at two to three months of age, with intervals of six weeks. A vaccination was to be given along with pertussis in the second or third inoculation. Booster shots for pertussis and diphtheria were the sixth and seventh inoculations of the series. According to plan, an infant starting the series at three months of age would have inoculations for pertussis at 3, 4½, and 6 months, plus a vaccination at 4½ or 6 months, then inoculations for diphtheria at 7½ and 9 months, with booster shots following at 12 and 42 months.

The collection of data began in April of 1951 and was to continue until September of 1952. In the spring of that year it was decided, because of possibly increased susceptibility to infantile paralysis caused by any inoculations in the summer months, to postpone inoculations until the late fall. By that time the triple vaccine had replaced the previous methods of inoculation. Altogether about 2,000 records of infants who had one or more of the series of inoculations were available when the study was terminated. Of these, about 800 infants completed six of the total series of seven.

The fact that less than one fourth of the infants had gone almost the entire series at the closing date was not too disappointing since we still had a large number who had four and more inoculations, sufficient to include their first year of life, and longer, well within the age span required for the purpose, according to the printed instructions, "to get some idea of the age at which an infant first gives evidence of remembering inoculation as a painful experience."

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[3] Dr. Regine K. Stix served as research consultant. She supervised the record forms and briefed the personnel of the health stations.

The main difficulties in utilizing the records were due to various exigencies arising out of a superimposed task on physicians with crowded schedules, on certain variations from the expected uniformity of their procedures, on accidental factors including illnesses and absences, cancellations of appointments, and postponements or neglect in bringing infants for a first inoculation until they were six months of age or older.

To learn when a first memory of an inoculation occurred we hoped, at least, to keep the external events uniform, within certain limits. We could not expect under the circumstances nor did we attempt to have every mother hold the baby in the same manner or to have every physician adhere rigidly to the same technic and speed in performing the immunization.

Numerous variables in the baby's experience of inoculation, as they were observed in the examining rooms, are listed in detail in Appendix II. They show clearly enough the wide variations in what is supposedly a simple routine procedure.

On the whole the physicians were careful in listing dates of inoculation, types of immunization, and in checking replies to questions on the child's behavior, mother's "excessive" reactions, and to the nine questions dealing with the time of the baby's cry whenever a cry occurred.

Of the final two questions, one was used as a check on the time when the cry first occurred ("when did baby first cry in examining room?"). The final question was used chiefly to maintain the physician's interest in the investigation and also elicit his opinion as to the baby's memory ("do you think this child remembered a previous inoculation? Check yes, no, or no opinion."). No questions were asked concerning the intensity or duration of the cry. A few physicians however characterized the cry when it was of very brief duration (most frequently during or after a vaccination). Rarely a physician elaborated his opinion that the child remembered a previous inoculation by writing down his observations on the back of the record sheet as we requested. A frequent omission in the record were the notations called for in Row I, dates and details concerning injections given outside the health station. When such omissions occurred, the records were less useful for studies of memory. They were still useful for other studies; for example, the number and type of inoculations that were not followed by cries, "excessive reactions" of mothers, etc.

C. SELECTION OF RECORDS

Our purpose in this investigation is to utilize the data in the records not only for the study of first memories and other relevant findings that bear on the question of inoculation-memory, but also to learn how to exploit records of this type which have varying degrees of usefulness.

Selections of the records can be made in various orders of stringency. The first selection requires the highest feasible degree of uniformity, considering the limitations of the records. The criteria (which include healthy children for all selective groups) are as follows: (*a*) The infant is always accompanied by the mother and (*b*) on her lap when inoculated. (*c*) The doctor wears a white coat. (*d*) First inoculations at 4 months of age or younger; (*e*) a series of 4 or more; (*f*) intervals of one or two months for the series of 4; (*g*) with pertussis in the first 3; (*h*) including one vaccination in the second or third; (*i*) and followed by inoculation for diphtheria in the fourth and fifth. (*j*) A physical examination precedes each one of the first four inoculations and (*k*) each inoculation is followed by a cry. (*l*) No cry occurs during the physical in the first four of the series. (*m*) No evidence of disturbed behavior or a cry until inoculation in the first and second session, and after the physical examination in the third session. (*n*) No evidence of "excessive reaction" on the part of the mother. (*o*) No evidence, as stated in the record, of inoculations outside the station, or transfusions, hospital experience, surgical intervention (excepting circumcision in the first month of life). (*p*) The same doctor is in attendance during each inoculation of a series.

The reasons for the criteria may now be stated. (*a*) A change in the accompanying adult creates a special unknown factor which is best eliminated. Records in which the infant was brought in by friend or relative, as noted, were excluded. (*b*) The rule of performing inoculation when the infant was on the mother's lap was adhered to in all cases with few exceptions. The requirement that all inoculations be made on the arm was followed in our series without exception. (*c*) In two stations, physicians did not wear white coats. That was their custom and we favored it, to find out if it affected the frequency of "memory" cries. No significant difference was found. However, data on the fear of a white coat were not recorded. Data derived from these stations were not included in the selected series of cases that follow. (*d*) The first inoculation starts most frequently at 3 or 4 months. We could with a large number of cases and a higher degree of uniformity as to intervals, and other criteria, compare memory differences based on the age at first inoculation. Since on the basis of previous studies we found no evidence of memory of inoculation in the first six months of life, it was thought that criterion (*d*) was a safe one.

The official procedure for inoculation at the time this study was made is given in Table 1.

TABLE 1

	Age	Inoculum	Quantity
1st inoculation	3 mos.	Pertussis Vaccine	1 cc.
2nd "	4½ " plus small pox vacc. (multiple pressure)	" "	1.5 cc.
3rd "	6 mos.	" "	1.5 cc.
4th "	7½ "	Diphtheria (Alum Ppt. Toxoid)	1 cc.
5th "	9 "	Diphtheria	1 cc.
6th "	12 "	Alum Ppt. (Pertussis Booster)	1 cc.
7th "	42 "	Diphtheria Fluid (Toxoid Booster)	0.2 cc.

(Syringe 2 cc; Needle ⅝ — 25 gauge)

(*e, f, g*) We had few records in our series of infants who were inoculated exactly at all the ages set down in the recommended procedure. As stated previously, this fact was not disconcerting as long as we had a sufficient number of records of inoculations for the first year of life. The first three of the series were fairly uniform but failed in the vast majority to satisfy other criteria. Intervals of time between inoculations, as long as the first inoculation began at four months or earlier, were accepted and duly noted.

Ideally we should require the same age at onset and the same interval of time between each inoculation in the series. The intervals were accepted as of one or two months, since it was found by a trial-and-error study that no evidence of memory occurred in a single instance in this investigation as in the preliminary one in the first five months of life, regardless of the length of the interval or the highest frequency of inoculation periods as shown in our records.

The reason for requiring a series of at least four inoculation periods (counting inoculations for pertussis and smallpox when given in the same session as one period) is a protection against a false inference of a "memory cry." The protection is by no means an absolute one, however. Limiting the proof of memory as in this investigation to the cry alone, we are able to say that under similar conditions no cry occurred until the experience was repeated a number of times. Suppose an infant went through an entire session without a cry or any other signs of untoward behavior on three successive occasions until the insertion of the needle. Now suppose that on the next, the fourth occasion, he began to cry some seconds before the needle was inserted, at the time his arm was swabbed with alcohol, or even before then, when the physician was preparing the syringe. We would then infer that the cry was probably not fortuitous, that it might very well be due to the infant's anticipation of the painful event, the next thing in a precise series of happenings experienced three times before. Since a cry in any session is more likely to take place during a physical examination, we required a series of four sessions without a cry during the physical examination before considering a cry at that time to be inferential of memory.

There are other ways of determining the validity of inference that cries occurring at certain times are likely evidence of memory of an inoculation. That will be considered in a later section, but first we should be concerned with cries that occur during sessions at the health stations that have no possible causal relation to inoculations.

At four health stations all infants and children were checked for crying and age in months, over a period of one week respectively. Cries following an inoculation were not recorded. The number and percentage of infants and children who cried once or oftener are arranged in Table 2 in six-month periods up to 36 months. The remainder are lumped together.

TABLE 2
NUMBER OF INFANTS WHO CRIED ONCE OR OFTENER WHILE IN THE EXAMINING ROOMS OF FOUR HEALTH STATIONS: CRIES FOLLOWING INOCULATIONS WERE NOT COUNTED ($N = 468$)

Age in months	Number of infants	Number who cried	Per cent
0 to 5	523	136	26
6 to 11	418	92	22
12 to 17	102	44	43
18 to 23	76	35	46
24 to 29	76	42	55
30 to 35	52	26	50
36 to 72	1860	93	05

Note the general increase in frequency up to three years of age and the large drop in frequency for the older age group. Since all the crying as tabulated precedes inoculation during any session, the rise in frequency with age could be utilized as proof of memory if we had for comparison large numbers of uninoculated infants and children. Such comparisons are not available.[4]

---

[4] Within the group of 468, only the four-month-olds allowed a comparison with

Actually, judging by the assumed reasons recorded for crying in the first year of life, most of the cries are explainable as reactions to the physical examinations, chiefly to throat and ear examinations. Other assigned reasons include physical manipulation of any kind, oppositional behavior, fear of strangers, fear of inoculation, illness, hunger, and fatigue.

The table reveals the obvious finding that much crying goes on in the health station that has nothing to do with inoculation. In regard to "memory cries" of inoculation, the marked drop at three years and later would indicate a reduction also in "memory cries." This may be due to extinction of memory and/or to manifestations of memory by methods other than crying.

Of special relevance to this study was the finding that crying may occur at any one of the stages thought possibly to indicate memory of inoculation; at the time the arm was wiped, at the time of preparation, etc. Thus 14 three-month-old infants with a previous inoculation cried at one of these stages. None of them cried at any one of these stages at a later inoculation following the same span of time, when presumably they would be more likely to remember, being older and reinforced by another experience. On the basis of such findings, the criterion of a minimal series was established.

A study of cries preceding inoculation was made also of infants at their first inoculation session in the health station. A sample of 500 immunization records was taken seriatim, excluding, however, cases who had previous inoculations or operations outside the health station. Such information, however, was sometimes missing (an error of 2 to 3 per cent in the majority of these cases). Table 3 can yield therefore only an approximate figure for infants who had previously no experience with needle insertion. The table includes the cries at their first occurrence during the session.

The percentages in the last column are arranged for comparison with Table 2. The lower percentage for the 2 to 5-month group may be due to the omission of infants in the first two months of life, whose frequency of cries was higher than infants age two to 5 months. The percentages for the group age 6 to 11 months are very close.

Note the number of late comers for immunization. In the sample of 500, as many as 87 (17 per cent) started as late as five months; 3 as late as 13 months. The large majority (83 per cent) began their immunizations at four months of age or earlier.

Note also that the majority of cries listed in the table occur during the

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over a dozen in each group. There were 33 in all. Of these, 16 had a previous inoculation. Four of them cried at one time or other during the session. Seventeen had no previous inoculation. Three of them cried.

TABLE 3

CRIES PRECEDING FIRST INOCULATION (N 500)

Age in months	No. of cases	Cry at entry	Cry during physical examination	Cry at preparation	Cry at alcohol swab	No. of cries	Per cent
2	17	0	3	0	0	3	10
3	261	5	18	1	3	27	
4	135	0	9	0	3	12	
5	44	1	2	0	2	5	
6	14	0	1	0	0	1	23
7	14	0	2	0	1	3	
8	4	0	2	0	0	2	
9	2	0	1	0	0	1	
10	2	0	0	0	0	0	
11	2	0	1	0	0	1	
12	2	0	1	0	0	1	
13	3	0	1	0	0	1	
Total	500	6	41	1	9	57	

physical examination (70 per cent). Only one of the 57 cries began when the infant was in his mother's lap before the arm was swabbed with alcohol ("cry at preparation"). The cry at this time, therefore, the time just after the physical examination when the baby is back in his mother's lap while the physician is getting ready, would appear to be the moment when a cry may be inferred to be a "memory cry" in a higher proportion of cases than at any other moment in the preverbal stage.

Note how the criteria of selection are used to solve the problems presented. They eliminate the late-comers (age 5 months and older). They use the cry at the time of the physical examination as probable evidence of memory only when in the hands of the same physician the infant had not cried at that time in his first four sessions.

Cries beginning during the physical examination may stop when it is over, or continue to the end of the session. Of 41 such cries listed in the table, 25 stopped and 16 persisted. It was thought that a "memory cry" starting during the physical might be more persistent than other cries, at least in the later months. The evidence, when all cries during the physical examination were listed in every inoculation session, is scanty. Thus of 50 cries in the 2 to 5 month group, 38 per cent were persistent; of 97 in the 6 to 11 month group, 39 per cent; of 100 cries in the 11 month and older group, 50 per cent.

(*h*, *i*, *j*) The order of inoculations was held constant for our first selection. Any variation changes the similarity of experience in regard to sequence and to potential pain. A diphtheria inoculation is presumably more painful than inoculation for pertussis (see Appendix II). Vaccinations are the least painful. A variation in procedure that allowed inoculation for pertussis and diphtheria in the same session or of vaccination alone would alter the similarity of "external events."

(*k*, *l*) For our first selection we required a physical examination in every case in the same place in the order of events; that is, preliminary to inoculation. We required also that no cry occur during the physical examination in the first four of the series of inoculations. Since the physical examination is the most likely source of cries, other than inoculation, it was thought more prudent to require a longer series of undisturbed physical examinations to lessen the chance of a false inference that a cry during that procedure is a cry awakened by anticipation of the forthcoming inoculation.

(*m*) A cry occurring after the physical examination but preceding the inoculation is used as inference of memory in the third inoculation period.

(*n*) The requirement that pain as proven by a cry after each insertion of the needle occurs in the first four periods is an obvious criterion, otherwise there may be nothing painful to remember.

The number of records that were not included on this account ran as high as 22 per cent in some stations. Since vaccinations were always combined with inoculation for pertussis, the rule was not applied when a cry followed pertussis inoculation and did not follow vaccination. As long as any inoculation was followed by a cry in the first four, that item was accepted (Table 4).

TABLE 4
ABSENCE OF CRYING FOLLOWING IMMUNIZATION: SAMPLE STUDIES OF FREQUENCIES OF "NO CRIES" FOLLOWING IMMUNIZATIONS FOR PERTUSSIS, DIPHTHERIA, AND SMALLPOX

Physician	Number of inoc. pert.	No cries	%	Number of inoc. diph.	No cries	%	Number of vacc.	No cries	%
A	280	33	12	123	3	2	101	23	23
B	311	40	13	155	9	6	106	55	52
C	243	30	12	73	0	0	91	48	53
D	296	50	17	146	8	5	74	12	16
E	417	43	10	199	2	1	135	30	22
F	266	21	8	140	0	0	76	8	11
G	295	4	2	161	0	0	92	21	23
H	287	33	11	104	3	3	100	68	68
Total	2,395	254	11	1,101	25	2	775	265	34

Elimination of cases in which the infant was noted as "disturbed" on entering the examining room in the first three sessions of inoculation was regarded as a necessary precaution. An inoculation during such a period, or even just preceding such a period, is presumably a different experience than an inoculation under ordinary circumstances. The requirement of "absence of a cry" until the inoculation in the first three includes almost all the "disturbed" cases since very few were so regarded in the absence of a cry in the first 12 months. In later months some children showed obvious disturbances without crying, by excitement, painful expressions, unruly or restless behavior, and verbalizations (e.g., "no needles, no needles!").

(*o*) The requirement that no case is accepted in which the mother shows "excessive reaction" is necessary to keep the infant's experience uniform. There are very few cases of that sort, not enough to show any effect of such reactions on the memory of inoculation. Typical of "excessive reaction" is that of a mother who refuses to hold the baby during his immunization.

(*p*) The requirement that no case be included who has had inoculations outside the health station (usually penicillin), any insertion of the needle, or surgical procedure, etc. (except circumcision in the first month of life), is clearly a necessary one in our first selection of cases, if we wish to obtain evidence of first memories that arise from experience of inoculations at the health station alone. The number of such records in a sample of 500 was 66 (13 per cent) (Table 5).

TABLE 5
INOCULATIONS, OTHER INSERTIONS OF NEEDLES, AND SURGICAL PROCEDURES OUTSIDE THE HEALTH STATION: SAMPLES OF 100 INFANTS EACH IN FIVE HEALTH STATIONS (AGE 1 TO 18 MONTHS, $N = 500$)

	Number of cases			
Station number	Penicillin	Globulin or BCG vaccine	Blood count	Circumcision after one month of age
1	7	2		1
2	8			
3	8		1	1
4	14			
5	24			
Total	61	2	1	2

Less than three records per 100, out of a total of over 2000, satisfied the criteria used for our first selection. Elimination of the largest number was due to neglect in furnishing information for Item I, "injections outside the health station." The next most frequent reason was change in the sequence of inoculations and physical examination. The third was the absence of a cry following inoculation.

(*q*) It was required that the same doctor attend each child through his series of immunizations. A "strange" doctor might presumably create a special problem if the child were aware of the difference in the doctor's appearance or in his technique. If that were ever the case, we had no wav of proving it. Nevertheless this rule was maintained for the first and second selected group of cases.

(*r*) All infants were presumably healthy. Infants with colds, sore throats, or any evidence of infection are not inoculated. This rule need therefore not apply to our criteria since it is axiomatic at health stations. However, we excluded records of infants who were inoculated if they revealed evidence of mental deficiency, hydrocephalus, or other organic non-infectious aberrancies. Such cases were rare.

D. "ALL CRITERIA" CASES

Only 52 cases were found to satisfy all the criteria described (2.6 per cent). Table 6 contains all the information necessary for our purpose. Case 1 for example was inoculated at 3, 5, 6, and 7 months, four inoculations in all. On each occasion a cry occurred only after insertion of the needle. According to the criteria selected, a memory cry could be counted at the third

TABLE 6

"ALL CRITERIA" CASES: ORDER OF INOCULATIONS, AGE IN MONTHS, AND THE EVENT WHEN CRY OCCURRED ($N = 52$)

Case No.	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	Comments
						Age in months										
1		*		*	*	*										* = Cry after insertion of needle
2		*	*		*	*										
3		*	*		*	*										
4		*	*		*	*										
5		*	*		*	*										
6		*	*	*		*	*									
7		*	*		*	*	*									
8		*	*		*		*									
9		*	*			*		*								
10		*		*	*		*	*								
11		*		*	*		*	†								† = Cry during physical exam
12			*	*	*		*	*								
13			*		*	*		*								
14		*		*	*	*		*								
15		*	*		*	*		*								
16		*		*	*			*								
17		*		*	*	*		*								
18		*		*	*	*		*								
19		*		*	*		*	*								
20		*		*	*	*		*								
21		*		*			*	*								
22			*	*	*				*							
23		*		*	*		*		*							
24	*			*		*	*		*							
25		*		*	*			*	*							
26		*		*	*			*	*							
27		*		*	*			*	*							
28			*	*		*	*		*							
29			*	*				*	*							
30		*	*		*	*			*							
31		*		*	*		*		X							X = Cry on entry and through physical exam; then stopped and then cry on wipe
32		*	*			*			*	*						

TABLE 6 (*Continued*)

Case No.	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	Comments
33			*	*				*		*						
34			*	*		*				*						
35		*		*			*	*		*						
36		*			*		*			*						
37			*		*		*	*		*						
38		*	*		*		*	*		*						
39		*		*				*		*						
40		*		*	*			*		†						Cry during physical exam
41		*			*			*		*	‡					‡ = Cry when arm was wiped at 12 months
42			*		*		*			*	*					
43		*		*		*		*			*					
44		*				*			*							
45		*		*	*			*			†					Cry during physical exam
46		*		*		*		*					*			
47		*		*			*	*					†			Cry during physical exam
48			*	*					*					*		
49			*		*	*			*	‡				*		Cry on wipe at 11 months
50			*	*				*	*	*					*	
51		*	*	*							*				*	
52			*	*	*				*		*				*	

Intervals in days between "memory" cry and previous inoculation

Case no.	
11	42 days
31	49 days
40	65 days
41	21 days
45	89 days
47	140 days
49	42 days

session, at 6 months, if it occurred after the physical examination and preceded the inoculation (cry "at wipe" or cry "at preparation").

Consider Case 11. At the fifth session of inoculations, the infant cried during the physical examination. He had not cried during the physical examination or at any other time excepting after insertion of the needle in his previous four sessions (at 3, 5, 6, and 8 months of age); and also, he never failed to cry after each insertion of the needle. It is quite possible that his cry at nine months was not a "memory cry" at all, but a response to manipulation during the physical examination. We have at this age also the possibility of response to the physician, now first perceived as a stranger, and also, though rather early for his age, as oppositional response. Awareness of the doctor as a stranger, however, would have aroused a cry at entry. Oppositional behavior is accompanied by a struggle, which was absent. Besides, as can be seen on the chart, he was the only one of 13 nine-month-olds with four or five previous inoculations who had this reaction. His previous inoculation took place one month previously. All we can say is that a "memory cry" is a fair inference.

Case 31 reveals a cry at a different point in the sequence of events during an inoculation session. Crying began at entry and continued throughout the physical examination. The infant stopped when his mother took him in her arms and sat down. He started again some seconds before the needle was inserted, immediately after his arm was wiped with a pledget of cotton soaked in alcohol. Assuming that a memory of something painful had been awakened when he entered the examining room, we would infer that his memory grew dim or that he was diverted on completing the experience of changes in posture and manipulation during the medical examination and also by the next step, enclosure in the mother's arms. A further sequence of events then followed that reawakened his memory; a postural change for the inoculation, the movement of the physician as he made ready, the smell of alcohol, the physician's arm as he swung around, etc. However, the cry did not return until he felt the swab on his arm, a few seconds before the painful event. His particular pattern, a cry as though a memory is present, to be lost and then to return just before the needle is inserted or just before that, while the doctor is in preparation, is fairly common. The other cries that began during the medical examination at 9, 11, 12, and 14 months (Cases 11, 40, 45, and 47) came to a halt when the examination was over and were not followed by another cry until the needle was inserted.

The cry "at wipe," noted for Cases 41 and 49, at 11 and 12 months of age, represent the earliest type of memories we found in previous studies when the selective criteria used for this group were not employed. They can be seen also in cases as young as 6 months when two inoculations are made, one for pertussis, the other for diphtheria. A cry occurs after the first inoculation. It may then stop and start again before the second insertion of the needle, at the time of the wipe. This is like a simple type of conditioned response experiment. The same infant whose cry indicates anticipation of the second needle may show no such evidence of memory for the first needle when it is inserted a month later. (Such cases, of inoculations for pertussis and diphtheria in the same session, were not included in our "all criteria" group.)

A similar example is seen in Case 49. The first memory sign was a cry at wipe when age 11 months. At the next inoculation this sign or any others are no longer present. The next inoculation, however, was five months later.

According to the criteria selected, the six-month-olds could be checked for a "memory cry" at a third inoculation session if the cry occurred at the time of "wipe" or "preparation" only. As explained previously, when a cry at entry or during the physical examination occurred during the first, second, or third inoculations, the record was not selected for this group.

Since the cry at "wipe" or "preparation," if due at all to a memory of the previous painful event, represents its simplest variety, since it is elicited so closely in time to the pain of inoculation, the criterion of selection is therefore not too rigorous. Of the infants inoculated at 6 months, a count of Table 6 reveals 27 who were in their third inoculation session. None cried at any time preceding the inoculation.

Note also that none of the infants who had three or more inoculations at the time of their latest record showed presumptive evidence of memory at 7 or 8 months (40 cases). One in 29 cases revealed a "memory" at nine months, one in 16 at 10 months, two in 13 at 11 months, and three in the remaining 12 cases at 12 months and older. As we advance in age from 7 and 8 months to 12 months and older, the percentage of "memories" proceeds as follows: 0, 3, 6, 15, 25. This finding at least indicates a definite trend in favor of the inference that we are dealing with memory cries, or at least that the artifacts (coincidence cries) have been much reduced by the criteria of selection, since the memories increase consistently with age. Furthermore, a glance at the chart will show that up to 12 months the memories came only one or two months after a previous inoculation and that longer intervals, of three and four months, occurred only in the group age 12 months and older (a 3-month interval in one case, age 12 months, and a 4-month in one case, age 15 months).

Both findings, as to increased frequency with age and the intervals, are in favor of the inference of the factor of "memory cries."

E. All Criteria Except "a Physical Examination During Every Inoculation Session"

Our next selection of cases was made by eliminating the criterion (*j*) of a physical examination to precede every inoculation, and does not include the previous cases. Some physicians varied in this respect, skipping the medical examinations on one or more occasions, most usually on alternate inoculation sessions. Ninety-four records satisfied these criteria. The data are contained in Table 7.

Eliminating Criterion (*j*) required no change in Criterion (*l*), which states, "no cry occurs during the physical in the first four of the series." The latter criterion still held though the number of physicals was reduced to two or three.

Again, as in Table 6, we have a gradual increase in frequency with a jump around 12 months. Again the intervals of time that are longer than two months appear, with one exception, in infants of the older age group.

The youngest infant to reveal a possible "memory cry" in this group is six months old (Case 94). No indication of a memory cry occurred at the next inoculation four months later. It is possible that a memory of the inoculation at six months, occurring after a painful experience in the previous month, faded out in the long ensuing interval of time. If the fourth inoculation had come at seven months of age instead of 10, we would have had a check on the validity of such an inference.

In Case 63, a cry occurred during the physical examination at nine months and again in the next inoculation session at 11 months. Cries preceding insertion of the needle occurred for the first time in the fifth and sixth of six series of inoculations. The time interval following the experience of pain was two months respectively when each instance of "memory cries" occurred. If no cry had preceded inoculation at 11 months, the inference of a "memory cry" at nine months could not be made, since within this age span it is assumed that the infant at 11 months would more likely remember an event after the same interval of time than he would at the age of nine months and would give proof of it with a cry. The cry at 11 months is therefore a confirmation of an inference of a memory at nine months. Even so, the possibility remains in spite of the precautions set up by our criteria that in both instances the cries may be due to some other cause. The probability in favor of a memory is supported by the only other instances of confirmation in the table (Cases 93 and 94) for memories occurring at 12 months. (The inference of a memory at 10 months in Case 93 is considered doubtful because of the long interval of four months following the previous inoculation.)

When an infant cries during the physical examination presumably because he anticipates a painful event, why does he stop after the examination is over though the event is still to follow? All the sequence-patterns of the cry that begins during the physical examination are depicted in Table 7. We have (*a*), as mentioned, the cry that stops after it is over and remains in abeyance until the inoculation (e.g., Case 86), (*b*) a cry may begin again "at the wipe," as in Case 82, or (*c*) "at preparation," as in Case 91, or (*d*) the cry once started may continue throughout the session, as in Case 93. We may assume that the cry that starts and stops represents a flickering reminder of the event, as though once the infant cries he forgets what he is crying about. He may get another reminder or clue when his arm is dabbed with alcohol, or even before, when the doctor gets ready. If the memory is less impermanent, once he starts to cry he continues. The patterns should theoretically be correlated with age. A larger number of selected cases are required to establish this point. In Table 7 Pattern (*a*) first appears at nine months, Pattern (*b*) at 14 months, Pattern (*c*) at 16 months, but Pattern (*d*) is seen as early as 10 months.

Eliminating the criterion of "a physical examination in every session" may have influenced some of the findings in Table 7. The proof, however, would require a much larger series of cases. As compared with Table 6, in which no "memory cries" occurred at 6, 7, or 8 months, we have two instances of probable "memory cries" at 6 and 7 months. The shorter time that elapses between entry and inoculation when a physical examination is omitted might be offered as an explanation. However the chances for the occurrence of such a cry were even better since the number of infants was larger (125 with three or more inoculation sessions per case for Ages 6, 7, and 8 months in Table 7; 68, in Table 6). Furthermore, the absence of a physical examination was not consistent. They occurred usually during alternate sessions. Finally the percentage of "memory cries" in both groups taken as a whole is close (5 and 6 per cent).

Table 8 offers a comparison of the findings in both groups. All cases six months or older with three or more inoculation sessions are included.

The combined table may be compared with the findings in the preliminary study referred to in Section *A*. In that study the first evidence of memory appeared at eight months (2 per cent of all cases). The percentages at 9, 10, 11, and 12 months were 4, 5, 25, and 25 respectively.

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TABLE 7 (*Continued*)

Case No.	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	Comments
94		*		*	†				*		†					X		At 6 months cry at preparation; at 12 and 17 months cry during physical exam and continued throughout.

Intervals in days between "memory" cry and previous inoculation

Case no.	
34	42 days
39	48 days
63	49; 77 days
78	42 days
79	64 days
82	83 days
86	141 days
91	42 days
93	125; 49; 167
94	42; 49; 167

TABLE 8

	"All criteria" cases (derived from Table 6)			All criteria except physical examination in every session (derived from Table 7)		
Age at time of inoc.	No. of cases	No. of "memory cries"	Per cent	No. of cases	No. of "memory cries"	Per cent
6 m.	27	0	0	41	1	1.6
7	22	0		50	1	
8	18	0		38	0	
9	29	1	7	36	1	5
10	16	1		25	2	
11	13	2		21	1	
12	5	2	25	16	2	23
13	0	0		10	2	
14	2	1		5	1	
15	1	0		4	1	
16	4	0		3	1	
17	0	0		2	2	
Total	137	7	5	251	15	6

TABLE 9

TABLES 6 AND 7 COMBINED (N = 146)

Age at time of inoc.	No. of cases	No. of "memory cries"	Per cent
6 m.	68	1	1
7	72	1	1
8	56	0	0
9	65	2	3
10	41	3	7
11	34	3	10
12	21	4	20
13	10	2	26
14	7	2	
15	5	1	
16	7	1	
17	2	2	
Total	388	22	5.7

F. INTERVALS

The time elapsing between inoculations would naturally affect the memory of them. In Table 10 the cases are all arranged according to time-intervals in months between an inoculation at a given age and the previous inoculation. The first row, for example, includes all 88 infants inoculated when age six months. Of these, 40 had been inoculated one month previously; 43, two months previously; 5, three months previously. One of them, as indicated in a corner of each box, had a "memory cry." At seven months, one "memory cry" occurred after an interval of one month, none after a longer interval.

TABLE 10

INTERVALS (FROM TABLES 6 & 7)

"Memory cries" in relation to time elapsing in months between an inoculation and the previous inoculation. Number of "memory cries" in lower right corner.

Age in months	Intervals in months 1	2	3	4	5	6 m. & longer	Total	Percentage of "memory cries"
6	$40/_1$	$43/_0$	$5/_0$				$88/_1$	1
7	$38/_1$	$24/_0$	$6/_0$	$2/_0$			$70/_1$	1
8	$27/_0$	$24/_1$	$5/_0$	0			$56/_1$	2
9	$14/_0$	$22/_2$	$18/_0$	$6/_0$			$60/_2$	3
10	$14/_0$	$15/_2$	$7/_0$	$6/_1$	$4/_0$		$46/_3$	6
11	$4/_1$	$15/_2$	$12/_0$	$3/_0$	0	$1/_0$	$35/_3$	8
12	$5/_1$	$7/_2$	$7/_1$	0	0	$3/_0$	$22/_4$	18
13	$4/_0$	$3/_2$	0	$1/_0$	$1/_0$	$1/_0$	$10/_2$	20
14	$1/_0$	0	$1/_0$	$2/_1$	$2/_1$	$1/_0$	$7/_2$	29
15	0	0	0	$2/_0$	$3/_1$	0	$5/_1$	
16	0	$1/_1$	0	$3/_0$	$2/_0$	$1/_0$	$7/_1$	
17	0	0	0	0	$2/_2$	0	$2/_2$	

We would expect a higher frequency of memory signs after a shorter interval of time. Originally, intervals were computed for the entire group of 2000 on the basis of calendar months, for the purpose of quick calculation and the usual assumption that in such a large number of cases it was a safe practice. Thus the interval in time for inoculations on April 1 and May 20 was counted as one month. When the criteria of selection reduced the number of cases so drastically, the same system of calculating intervals was still employed. Its effect would be most telling in the intervals of one and two months. Even so, and regardless of the small number of cases as the age in months rises above 10, the table is quite consistent with the assumption of increased frequency in memory and increase in memory over longer intervals of time with increasing age.[5]

Thus the first memory after an interval of one month is at age six months; after an interval of two months, age eight months; after an interval of three months, age 12 months. Not counting an exceptional and presumably doubtful case at age 10 months, the first memory of a four month interval occurs at age 13 months. Excepting the one case at 10 months (which therefore seems doubtful), there is no memory sign after an interval of three months or longer for any cases until age 12 months.

Since the total frequencies at each age level are determined in part by the varying intervals, a comparison was made for memory signs for intervals of one and two months alone. Starting at six months, the frequencies in percentage up to 12 months run as follows: 1, 2, 2, 6, 7, 11, 17. For 13 to 17 months, the frequency is 33 per cent.

G. "PENICILLIN CASES"

In this section all records are included which contained information concerning inoculations or operative procedures outside the health station, and at the same time satisfied all other criteria excepting the requirement of a physical examination in every inoculation session, and also the criterion concerning the cry at physical examination. Sixty-nine cases were found suitable for inclusion.

The reason for omitting the requirement "no cry during the physical exam in the first four of the series" was to make allowance for the effect on the infant's memory of penicillin inoculations that preceded inoculation at the health station at any time. As Table 11 reveals, regardless of procedures involving the needle, no evidence of a cry during the physical examination, or other evidence of a memory cry, occurred until the age of seven months.

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[5] For the first five of the entire series of inoculations (see Table 1), intervals of six weeks were recommended. They were attained within five days of that interval in the large majority. A sample of 500 "one month" intervals yields 42 ± 5 days in 86 per cent, 42 ± 10 days in 96 per cent. The range is 24 to 59. For Tables 6 and 7, the range is 31 to 49, the percentage of 42 ± 5 days is 85.

Of interest is the finding that in this group penicillin injections were given a week before the 7-month inoculations at the station in three cases (Nos. 3, 15, 34). Only one of the three revealed a "memory cry." At the same age two infants were injected with penicillin two weeks previously. Neither had a memory cry (Cases 35, 49). There remain five cases in which penicillin was injected three to four weeks prior to a 7-month inoculation (Cases 22, 23, 25, 31, 32). Altogether, of the 10 infants who had penicillin injections within a month of their inoculations at seven months, one only revealed evidence of a "memory cry" and in that case the interval of time was one of the shortest.

Of the 6-month-olds, 19 had had penicillin or other experience with needle insertion previous to inoculation at the station. None of them showed any evidence of a memory cry.

TABLE 12

DERIVED FROM TABLE 11: ALL CASES WITH THREE OR MORE INOCULATIONS INCLUDED, IN OR OUTSIDE THE HEALTH STATION

Age at time of inoc.	No. of cases	No. of "memory cries"	Per cent
4 m.	9	0	
5	18	0	
6	27	0	6
7	33	3	
8	31	2	
9	26	4	14
10	24	4	
11	22	2	
12	11	2	41
13	10	4	
14	2	0	
15	4	3	
16	4	3	
17	1	1	
Total (6 months and older)	195	28	14

Table 12 reveals a higher percentage of frequencies of memory cries than the previous tables. If we arrange the age periods for the "penicillin" cases and the others in the same way, the results are as given in Table 13.

Increased frequency of "memory cries" is presumably due to injections outside the health station. They are also presumably due, at least as applied to our series, to outside injections administered at six months or later, since we have no evidence of "memory cries" until age seven months.

TABLE 13

Age at time of inoc.	Cases "all criteria" and others derived from Tables 6 and 7			Penicillin cases derived from Table 11		
	No. of cases	No. of "memory cries"	Per cent	No. of cases	No. of "memory cries"	Per cent
6, 7, & 8 m.	196	2	2	91	5	6
9, 10, & 11 m.	140	8	6	72	10	14
12 to 17 m.	52	12	23	32	13	41
Total	388	22	5.7	195	28	14.4

When we subtract those "memory cries" during inoculations at the health station whenever they were preceded by an "outside" injection (with no intervening immunization) the reduced numbers are as given in Table 14.

TABLE 14

Age in months	No. of cases	No. of "memory cries"	Per cent
6, 7, & 8	91	4	4
9, 10, & 11	72	5	7
12 to 17	32	6	19
Total	195	15	7.7

The frequencies are now much closer to those derived from Tables 6 and 7. The findings lead to the obvious inference that the signs used for memory of a painful event due to insertion of the needle are more frequent when such events are more frequent. Nevertheless our first evidence of the influence on the "memory cry" of outside injections did not occur until seven months of age. The finding that previous "outside" inoculations had no effect on "memory cries" at six months or earlier as seen in Table 11 is reversed at age 13 months or older. Then in 6 of 7 instances in which an inoculation at the station was preceded by an inoculation outside the station, a "memory cry" emerged (Cases 55, 56, 64, 65, 67, 67).

Since we are dealing with episodic painful events with intervals of about six weeks in the vast majority of cases, our findings would naturally be altered if we studied the results on memory of repeated daily inoculations. It would be interesting to collect cases in which the inoculations at the station were interspersed with varying numbers of daily inoculations. Such data are not at hand.

The tables clearly show a rise in the frequency of "memory cries," starting at seven months with a spurt at one year of age.

When the "penicillin" cases are added to the others for the study of "memory cries" in relation to intervals, we have Table 15.

H. Summary and Comments

In this study the infant's memory of inoculation was investigated by means of the infant's cry. The purpose originally was to find a "safe period"; that is, a particular age in infancy during which inoculations, given according to procedures then in use at New York City health stations, would not be followed by any memory of them. It was assumed for the ages under consideration, the first two years of life, that the infant who remembered a previous inoculation would cry in anticipation of this painful event, when brought back to the place where it occurred.

About 2,000 records of serial inoculations were gathered in a number of health stations in New York City in the years of 1951 and 1952. Besides notations of age at the time of each inoculation, the particular inoculum employed, and the exact time of a cry in relation to the sequence of events in the examining room, the data included a number of other items, such as the mother's behavior, the infant's exposure to the cries of other children, and other inoculations or painful events experienced outside the health station.

The main problem of the investigation was how to determine the meaning of a cry when it preceded the inoculation. Was it due to anticipation of a painful event or to some other cause? It was known, for example, that a cry that has nothing to do with a previous inoculation (as in the very first one) may occur at any time preceding it. To overcome or at least reduce this obstacle, a first and rigorous selection of records was made, involving a minimal series of inoculations, without any cry until insertion of the needle, and elimination of all records containing any omission of data that would alter certain requirements of comparable conditions for each infant.

Based on the numerous criteria chosen for this purpose, only 52 records were satisfactory. By eliminating one of the criteria (a physical examination in every clinic-session), a second group, containing 94 cases, was selected. Finally a third group, which included infants who received inoculations other than immunization outside the station, besides operations or other experiences involving painful manipulation, were also studied. The third group, 69 cases, satisfied the other criteria required for the second.

Each group was studied separately and compared with the others. The "penicillin" group showed the highest frequency of "memory cries." When a correction was made for those cases in which a previous inoculation or

TABLE 15

INTERVALS: MEMORY CRIES IN RELATION TO TIME ELAPSING BETWEEN AN INOCULATION AT HEALTH STATION AND ANY PREVIOUS INOCULATION (FROM TABLES 6, 7, AND 11) $N = 215$

Age in months	Intervals in months						Total	Per cent
	1	2	3	4	5	6 m. & longer		
6	51/1	62/0	9/0				122/1	1
7	59/4	37/0	11/0	2/0			109/4	4
8	42/1	34/1	11/0	0			87/2	3
9	26/4	35/2	17/0	12/0	1/0		91/6	7
10	23/3	16/2	10/1	7/1	6/0	3/0	65/7	10
11	14/3	19/2	17/0	4/0	2/0		56/5	9
12	13/3	10/2	8/1	0	0	3/0	34/6	19
13	5/1	9/5	1/0	2/0	2/0	1/0	20/6	30
14	1/0	1/0	1/1	2/0	2/1	2/0	9/2	41
15	2/1	0	0	4/2	3/1	0	9/4	
16	0	3/3	1/1	4/0	2/0	1/0	11/4	
17	0	0	0	1/1	2/2	0	3/3	

operation outside the station preceded the finding of a memory cry at the health station, the frequency of "memory cries" was similar to the others.

With one exception (at six months) "memory cries" of inoculation were not found in the first six months of life. This finding was not altered by the fact that 21 of the 34 six-month-olds in the third group had received previously, besides immunizations, one or more injections of penicillin or insertions of a needle for other procedures.

There was a rising frequency of memory cries, when inoculations were given for immunizations according to public health regulations at the time of study, starting at one per cent at six months of age and rising as high as 20 per cent at 12 months. Up to age seven months, the findings were not altered by additional inoculations outside the health station.

Since the frequencies depend on the interval of time elapsing between inoculations, the smaller the interval the larger the frequency of "memory cries." Up to eight months, no "memory cry" occurred after any interval longer than one month; up to 12 months (assuming a spurious cry at 10 months), no "memory cry" occurred after any interval longer than two months.

On the basis of our findings in this and a previous study, we may conclude that inoculations given at six-week intervals and completed by six months of age are most unlikely to be remembered. The inference follows that in the absence of memory for these early inoculations they furnish no source of anxiety for later events.

It follows also on the same line of reasoning that the present method, completing immunizations by six months of age in two inoculation-sessions, to be followed by booster shots after intervals of more than two months, appears to be psychologically a safe procedure.

Other aspects of the investigation may now be summarized and discussed.

It appears obvious that the weakness in this undertaking which involved the coöperation of numerous physicians and nurses was the lack of frequent conferences and supervision. This is particularly true for an investigation that adds further work to busy routine services. The largest number of omissions however were due not to lack of time but to failure in coördinating the work of nurse and physician in regard to clerical data. Nevertheless by means of selective criteria we can test the records and select those which measure up to the requirements we have in mind. Selective criteria would have been necessary even if all the records had been completed in perfect and uniform order, as explained in the text. A simple example is the elimination of the records of cases in which no cry occurred after insertion of the needle, when our purpose was to find evidence of memory of inoculations experienced each time as a painful event.

The same records which were eliminated for that reason were useful for another purpose—the study of painless inoculations and vaccinations. This study showed a wide variation, from zero in diphtheria immunizations to 68 per cent in vaccinations for smallpox. Painless inoculations for pertussis, judged by the absence of a cry, occurred over five times as frequently as inoculations for diphtheria. Since in these inoculations the same volumes of inoculum were employed, the difference was attributed primarily to a difference in painful properties, and secondarily, to differences in technic (in which sharpness of the needle is included). Frequencies of painless vaccinations were as high as 68 per cent in one physician's practice, as low as 11 per cent in another's. Such variations were presumably due primarily to differences in frequency and pressure of the point of the needle on the skin. Observations of technical performance lent weight to statements that "some doctors always get more cries than others." Skill in the physician's technic of inoculation apparently requires frequent review by supervising physicians in the health center.

A sample study of the number of infants who cried once or oftener in the examining room, not counting cries following inoculation, showed an increased frequency with age, when taken in periods of six months, up to three years, after which a marked drop occurred (from 50 to 5 per cent). Assuming an increasing number of memory cries with increasing age up to three years, the inference may be made that for the older children, their memory of previous inoculations has been extinguished or reduced and/or manifested in ways other than by crying.

In a study of inoculations and surgical procedures outside the health station, it was found that the number of children who had received one or more injections of penicillin varied, in five different health stations, from 7 to 24 per cent. The influence of these and other painful events outside the health station was studied only in relation to memory of inoculations as revealed in the health station.

Among the variables affecting the experience of the infant during inoculation-session the following were included: sounds heard and people seen in the examining room; procedures in examining room and variation in schedule of inoculations; mother's behavior while holding the baby, facial expression, general reaction; inocula and technic; the needle; infant's previous painful experiences, physical condition, sensitivity to pain and post-inoculation conditions; the physician; and variables other than technic.

I. APPENDIX: VARIABLES

In this investigation the basic data were derived from the infant's response to inoculation. We tried therefore to insure uniformity in the method of inoculation, or, in other words, to control a certain number of variables without departing too far from the routine of the health station. The control exerted had to be limited since otherwise the work of the health station would be disrupted by making excessive demands; for example, having an infant enter an examining room without hearing the cries of other infants, etc. The large number of variations in a supposedly routine procedure and our method of dealing with them are considered in the pages that follow.

1. *Examining Room*

No changes were made in regard to the number of adults or infants present in the examining room at the time of inoculation. An infant was often accompanied by mother and one or more siblings. Infant and one or more siblings were also inoculated during the same session. The order of inoculation varied. Sometimes an older, sometimes a younger child was the first, depending on convenience or other circumstance; though usually the oldest child was taken first. The infant was exposed to the cries of other infants or children emanating from within or without the examining room. On this point the record form requires checks (no, yes, unknown) to the following questions: "VI. While waiting for M.D. did child: *a.* witness an immunization? *b.* hear screaming from examining room?" The duration of time infant and mother were at the health center before they entered the examining room was not recorded.

2. *Schedule*

There was strict uniformity of method in regard to type and dosage of the inoculation, though not in regard to their order in the series. The time intervals also varied. These variations were due to cancelled appointments, postponements by physicians because of the baby's illness (usually mild), and the rule introduced in 1951 of having no inoculations during the summer. The variations in time intervals presented no special problem for the investigator. In fact they supplied useful data for the study of the influence on the infants' memory of the time elapsing between the inoculations. Variations in the order of inoculations are considered in a later section.

The elimination of the inoculations during the summer months of 1951 curtailed the number of completed series since the accumulation of case records had a definite expiration date.

The usual procedure in the examining room was first an interview with the mother, who sat with baby in her lap alongside the physician's desk, then a physical examination with baby on the table or on mother's lap as the physician preferred, then further conversation with the mother depending on her questions and the medical findings, and finally, when required, the inoculation. In a number of cases, especially when the inoculation was postponed for the reasons already stated, the infant was brought solely for an inoculation.

This variation has an interesting influence on the assumed anticipation of pain by the infant. Suppose the procedure in the examining room was invariable. Suppose the infant was always first in the mother's lap, next on the examining table, next on the mother's lap, and then inoculated. Suppose also this experience was repeated at monthly intervals five times. The infant would then have the best opportunity, because of the precise order of events, to anticipate the next step. If he never cried and showed signs of apprehension until the fifth experience, say at the age of nine months and at the moment preceding the inoculation, we might then infer that he anticipated the next event, or, in other words, that he remembered. When however the sequence of events is broken, sometimes he has an inoculation and sometimes not, sometimes an examination, sometimes not, there is less likelihood of his "remembering." We can, of course, select cases in which the sequence of events follow in a precise order, or approximately so.

3. *Mothers*

Mothers vary in their manner of holding the infant, in their reactions to the infant's experience, in comforting, and in other ways. All such variations may presumably affect the experience of the infant.

Mothers' "reaction" to the baby's inoculation was checked on the record as "neutral," "pained," or "excessive." This particular item was thought to be unimportant in its influence on the situation in the health station, since the frequency of "pained" facial expression and "neutral" facial expression showed no significant difference in correlation with the time of the infant's cry.

Of 1,000 entries, facial expression "pained" was checked 136 times, "excessive reaction" two times. As anticipated physicians varied considerably in noting "pained" expression, from 1 to 12 per cent in five samples. One mother who showed a painful expression in each one of the six series of inoculations told the doctor she did not want to hold the baby but was persuaded to do so. Nevertheless she held the baby "comfortably" in each instance. One mother had a friend hold the baby for her. Few mothers showed a painful expression on more than two occasions. Most of those who had a painful look showed it on one occasion, usually during the first of the series of inoculations.

The notation "excessive reaction" was checked rarely. It included such cases in which the mother ran out of the room, refused to hold the baby herself, or wept.

Mother's "handling of the baby" was checked on the record as "comfortable," "neutral," or "threatening or punishing." Most of the items checked were "comfortable." No differences were found in the frequency of cries that could be attributed to "neutral" (an adjective employed to cover such observations as uncomfortable, awkward, clumsy, etc.). "Threatening or punishing" behavior was never noted in the examining room.

In regard to uniformity of the infant's experience in the health station, there was little difficulty in handling these items. We needed to eliminate all cases in which the mother's reaction was "excessive," and cases in which the baby was held by someone other than the mother. A very small number was affected by this provision.

Other variables in the mother's behavior could not or were not taken into account. Turning the baby's head away, tightness of the mother's hold on the infant insofar as the tightness was not accounted for by checking the word "comfortable," also changes, if any, in the tenseness of the mother's limbs and abdomen when she felt pain at the time of the infant's inoculation, are examples. Such experiences presumably could intensify the infant's experience of the inoculation, and thereby represent a stronger stimulus than the stimulus of the pain caused by the inoculation alone.

The infants included in this study were all inoculated on the mother's lap.

4. *Pain*

The memory we are seeking rests on the repeated experience of pain due to the insertion of a needle through the skin and the inoculation of fluid containing toxoid. The full series comprised seven inoculations. The first three were pertussis, the next two, diphtheria. Booster shots of pertussis and diphtheria followed. Besides the inoculations mentioned, a vaccination against smallpox was done, usually during the second or third period, in addition to pertussis.

The variations in response to the inoculations include considerations of the technic employed, the sharpness of the needle, the inocula, their order in the series, their combinations, sensitivity to pain, its intensity, and post-inoculation distress.

a. Technic. In the technical procedures the uniform features were (*a*) size of needle and syringe, (*b*) type and quantity of inocula, (*c*) use of arm as site of inoculation, and (*d*) the alcohol swab on the arm before insertion of the needle or in vaccination the repeated slight needle pricks.

Of the factors enumerated, there were differences in the pressure of the swab of cotton, its size, the amount of alcohol with which it was saturated, and the time taken for this task. A few physicians did it very quickly. The inoculation followed with such dispatch that both swabbing and inoculation appeared to be almost simultaneous.

The record form requires a check mark to denote the time the infant cried in relation, among other things, to the swabbing ("the wipe"). When swab and inoculation are done so rapidly, it is difficult to know if the infant cried at the time of the swabbing or immediately afterwards but before insertion of the needle. The records of the physicians who worked so rapidly revealed a relative absence of cries "at the wipe." Since a cry at that time may reveal the first (and simplest) anticipation of the pain to follow, since it is closest to it, we inferred from the "cry at wipe" the earliest memory of the event. This particular "first step" in the memory of inoculation was lost in these records. In fact it was because of the absence of "cries at wipe" that special observations of the technic employed were begun.

At the other end of the scale were those physicians who used large swabs of cotton, exerted a bit more pressure with it, and made a long, slow wipe. They used more time also before inserting the needle. Their records, as expected, revealed a higher frequency of "cries at wipe."

In regard to the technic used in vaccination, the variations are largely in the speed and pressure used in the repeated pricks. A study may be useful in determining the influence of the technic employed and its success as measured by the necessity of a repeat. For our immediate purpose it is sufficient to note that the more painful method acts as a reinforcement of the painful stimulus of the pertussis immunization when both vaccination and pertussis immunization were done in the same session.

Speed in attaching needle to syringe and in filling the latter vary considerably. The thrust of the needle into the skin is made at various angles, usually with deft and sure insertions and at times with hesitation. Variations occur in the area of the arm chosen, in steadiness, and speed in infiltrating the tissue with the contents of the syringe and in the pull of the needle outward. All hesitations, slowness, and awkwardness may increase the strength of the painful stimulus.

On this point there is apparently no way of ensuring uniformity of procedure. There are variations also in the manner of holding the arm, and also in telling the mother what to do—and in the manner of doing so. Some physicians give the mother a number of directions, to hold the baby firmly, to take it easy, to grasp the baby's arm, to avert his head. Others go about their business quickly and expeditiously and give the mother no directions at all unless she appears apprehensive.

b. The needle. Though all needles were supposedly examined for sharpness, presumably they varied in this respect. To learn the significance of this variable a special series was studied in which a new needle was employed for each inoculation.

Uniformity of stimulus and of the infant's experience in regard to the sharpness of the needle can be controlled, at least partially, by selection of records according to special criteria. Routine checks were made on the records which allow differentiation of cries which began as soon as the arm was pricked from cries which began after the needle was inserted and at the beginning of, during, or after the flow of the inoculum. In most instances the cry occurred several seconds after the injection began. Whether the cry when not immediate is a delayed response to the pain of the needle rather than to the pain resulting from the flow of inocula is difficult to discern, except in those instances in which the cry apparently begins toward the end or just after the injection. In those rare instances when the physician noted that the needle he used was blunt (and therefore put aside), the cry occurred clearly at the time of insertion.

c. Inocula. A number of inoculations for pertussis and diphtheria are not followed by crying. The percentage of "no cries" varies with different physicians. There is nevertheless a consistent difference. A cry is much more likely to occur after an inoculation for diphtheria. The frequencies of "no cries" following inoculations for pertussis were 11 per cent, for diphtheria, 2 per cent (see Table 3). The volume of pertussis inoculum was 1 and 1.5 cc, of diphtheria, 1 cc. Hence the difference cannot be attributed to a difference in volume.

A vaccination was given usually in the second or third inoculation session along with inoculation for pertussis. On some occasions it was given on a fourth or fifth session along with immunization against diphtheria. On occasions also an infant's vaccination was repeated. Vaccination was rarely the only inoculation during a session (less than one per 100).

Of the three types of immunization, vaccinations were the most easily borne, judging by the absence of a cry (34 per cent in 775 cases). They were also the only inoculations in which physicians noted that some of the cries when they occurred were "very brief," or merely a "whimper."

Since vaccinations were the least painful of the inoculations, and often apparently painless, their particular order, as long as they were given along with a hypodermic inoculation (pertussis or diphtheria) was not controlled by selection. For the purpose of selecting a "uniform" series, we ruled out all cases in which an inoculation was not followed by a cry in any session, cases in which vaccination alone was performed, and cases also in which more than one vaccination occurred.

The order of inoculations was three for pertussis, two for diphtheria, with booster shots to follow in the sixth and seventh session. For our most uniform series we required that order of succession, with one vaccination, whether or not the series was completed at the time special recording was terminated.

In a few stations inoculations for pertussis and diphtheria were given during the same session. These records were weeded out according to the criteria of selection proposed.

5. *The Infant*

Some of the variables related to the infant's experience of inoculation that have not been included under the captions in previous sections of Appendix or in the text may now be considered. These have to do largely with the infant's experiences preceding and following an inoculation that bear specifically on the memory problem, and his exposure to the cries of other infants.

It is clear that an infant who has had a number of inoculations of penicillin daily followed by an inoculation at the station is in a different category from the infant whose experience of inoculation is limited to immunization at the health station.

The same applies to infants who have had any inoculation, operation, or hospital experience, or accidents involving cuts and bleeding. In our quest for uniformity of specific experience, we have separated such records from the others and have studied them separately. As stated previously, records from stations in which such information was not supplied could not be used for studies of memory. Of the experiences outside the station, judging by comparisons of the frequency of these notations in the records, when they were recorded, we have substantial data on inoculations (chiefly penicillin), surgical procedures, and hospital experience. Minor accidents like scratches or cuts or bruises, as expected in this type of investigation, were not elicited.

The physical condition of the baby at the time of inoculation was presumably normal. Infants with evidence of fever or respiratory infections were not inoculated. (When mothers wished to postpone inoculations for whatever reason, doctors usually acceded to their request.) The influence on memory of inoculations performed during an illness as contrasted with inoculations performed when the baby is well cannot be inferred from our records. They do enable us to tell if a first memory of an inoculation in the station was facilitated by the additional injections outside the station. From other case studies it is our impression that in certain cases a single injection is more likely to be remembered when the baby has a fever or discomfort from any source.

After an immunization some infants become fretful, inappetent, and have a rise of temperature lasting several to 24 hours. Mothers are apprised of such possibilities and appear to accept them in a matter-of-fact way. The influence of the sequelae of inoculations, which vary considerably, on the memory of inoculation cannot be studied in our records since they were not included. They were noted however in the medical reports and are available for study.

The infant's sensitivity to the pain of inoculation was not measured by the intensity or duration or absence of a cry. According to the criteria used for our first selection, we weeded out cases in which no cries occurred in any one inoculation. There are rare instances in which infants have had as many as four inoculations without a cry. This finding however may offer no proof of insensitivity. They may be variations due to chance and to differences in technic.

The record forms (see Appendix) contained the questions: "While waiting for M.D. did child (*a*) witness an immunization, (*b*) hear screaming from examining room?" The infant's opportunity to witness an immunization occurred when it was done to a sibling during the same inoculation session as his own, and also when he walked inadvertently into an examining room when the door was left open. The latter event is unusual and limited to toddlers. It would not be included in the record since the question was limited to the infant who was being immunized at the time. The observation "witnessing" means merely that an immunization of a sibling occurred in the presence of the infant.

In five samples of 100 records each from different health stations, there were altogether nine instances in which an infant "witnessed an immunization" and in one of these an infant had the experience twice. It is assumed that the infant who saw a sibling immunized also heard him cry. Cries heard from without the examining room were recorded for 86 infants, and of these on two or more occasions for 37. Six infants were in both categories. Altogether 89 infants witnessed an immunization or heard the crying of infants outside the examining room on one or more occasions (18 per cent).

The term "screaming" was not defined and, no doubt, the wide variations in the frequencies recorded in the five stations (1 to 49 per cent) were due largely to the inclusion of cries of different intensities, according to personal judgement.

One way of determining the influence of hearing a scream (or whatever the physician regarded as such), or of witnessing an immunization, or both, on the memory of inoculation, is to note the presence of a cry preceding inoculation in the following session and to compare their frequencies with infants who were spared that experience. In this comparison, age and intervals of time are kept constant. Thus for every infant who heard a cry or witnessed an immunization at seven months, for example, and who had his next inoculation at nine months, we selected the nearest case in the series of an infant who was inoculated at seven months, without such exposure, and whose next inoculation was also at nine months. Eighty-nine comparisons with matched intervals were available in our sample of 500 cases. They are shown in Table 16.

TABLE 16
INFANTS' CRIES IN INOCULATION SESSIONS THAT FOLLOWED INOCULATION SESSIONS IN WHICH THEY HEARD SCREAMS OR WITNESSED AN IMMUNIZATION OR BOTH

Age in months	No. of infants who heard screaming or witnessed an immunization before inoculation	No. who cried before inoculation in their next inoculation session	"Control group" (equated for number, age and intervals)
3	10	0	1
4	10	0	0
5	7	2	1
6	9	2	2
7	7	2	0
8	7	0	0
9	13	3	3
10	8	3	2
11	4	2	1
12	5	4	4
13	4	1	2
14	3	3	3
15	0	—	—
16	2	2	2
Total	89	24	21

An examination of Table 16 suggests, despite the fact that the number of comparisons is small, that the special experience of hearing screaming or witnessing an immunization before an inoculation session, has little or no effect on the incidence of crying. Up to 11 months, it might be noted, where the number of cases is adequate for purposes of comparison, the lack of difference between the control group and the "special experience" group, in regard to incidence of crying, is striking.

6. *Physician*

The physician's technic has been considered previously. Physicians as required wore a white coat, with two exceptions. The exceptions were planned for, to test out the difference in their effect on the memory of inoculation. No significant difference was found.

The effect of variations in the appearance of the physician, excepting the white coat, was not made a special subject for study.

By rule the same physician was to conduct all inoculation procedures for the same child. Some exceptions to this rule were necessary and have been referred to.

Differences in the manner of physicians with mothers, in their conversations with her, in their way of telling her what to anticipate as a result of inoculations, were not considered.

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CAUSES OF RETARDATION AMONG INSTITUTIONAL CHILDREN: IRAN*

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A. INTRODUCTION

Considerable interest has recently been shown in the fact that in some institutions for children there occurs a decided retardation in behavioral development. The observations of Spitz (8, 9, 10) in particular have received much notice, chiefly because of the interpretations which Spitz has placed upon his data. In our opinion, the primary importance of these observations lies in their challenge to the theory that infant development consists largely of the maturation of a motor sequence which is little affected by learning.

Aside from the investigations of Spitz, studies of behavioral retardation among institutional children have been few in number. The scarcity of such studies is due in large part to the fact that institutions in which conditions comparable to those described by Spitz can be found are not numerous. In many countries institutional care has been replaced by other methods of caring for dependent children. However, institutions in which behavioral development is retarded can still be found in countries which are "underdeveloped" not only in regard to modern technology but also in respect to newer methods for the care of foundlings and other homeless infants.

The present paper reports studies of development in three institutions in Tehran, the capital of Iran. In two of these institutions, children are exceedingly retarded in their motor development. In the third little retardation is present. It is believed that comparisons of child care in these institutions,

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[1] This study was conducted during 1958-1959 when the author was on leave from Brooklyn College and served as a visiting professor at the American University of Beirut, Lebanon. The investigation was made possible by a grant from the Rockefeller Brothers Fund to the American University of Beirut, and by a grant to the author from the Social Science Research Council. In Tehran, the assistance of Miss Gay Currie of the U.S.P.H.S. and I.C.A. was invaluable. The institutions in Tehran participating in the study were most coöperative; they have not been named in this report but can readily be identified by persons acquainted with the local scene. Special thanks are due to Mrs. B. Azuri who served as interpreter and research assistant. The manuscript has greatly benefited from the suggestions and criticisms of colleagues at the American University of Beirut and at Brooklyn College.

and of behavioral development in them, will throw considerable light upon the nature of the environmental factors which influence motor development. This paper supplements a recent report on behavioral retardation in a Lebanese institution by Dennis and Najarian (4). In the earlier report attention was directed primarily to motor development in the first year of life, whereas in the present instance the period from one year to four years of age is the one with which we are mainly concerned. Preliminary observations indicated that development during the first year in the two Iranian institutions in which retardation occurs is essentially the same as in the Lebanese institution described in the previous paper. For this reason in the present study attention is given chiefly to the age period to which little attention was directed in the earlier report.

B. Description of the Institutions

The two institutions in which marked retardation occurs, which will be called Institutions I and II, are supported chiefly by public funds; the third institution, to be labeled III, is supported by private funds. Several other children's institutions both public and private, exist in Tehran. The present report should not be taken to imply that retardation prevails in the majority of Iranian institutions.

It is worthy of note that the number of children to be found in institutions in Tehran is quite large. This number is explained by several factors. For one thing, Tehran is a large city, having approximately two million inhabitants. The recent growth of Tehran has taken place in the main through migration from villages. This has led to a considerable amount of social disorganization which has increased the number of illegitimate children, foundlings, abandoned children, orphans and half-orphans. Furthermore in Tehran at the present time, provisions for the care of dependent children, other than by institutionalization, are quite inadequate. Consequently, almost all children not living with parents or relatives are to be found in institutions.

1. *Institution I*

Institution I feels obligated to accept all foundlings and all abandoned children under three years of age who are brought to it. The population of the institution varies from day to day because of departures and admissions. During the time of the present study (September, 1958) the average daily population was about 600; of these about 275 were between birth and one year of age, 135 were between one and two years of age, and about 110 were between two and three years of age. While children above three years are generally transferred to other institutions, a few remain in Institution I beyond this age.

The excess of younger children over older children in Institution I may be due to several causes, including an increased intake rate in recent years, a higher death rate during the first year than in later years, return of older children to relatives, and transfer of older children to other institutions. The data at our disposal do not permit an assignment of relative weights to these factors.

More than nine-tenths of the children in Institution I are recorded as having been under one month of age at the time of their admission. When the actual date of birth is not known, an estimate of age at admission, based on weight, size, and appearance is made and placed in the child's record.

The mother never accompanies the child to Institution I nor sees him after admission.

In general children are placed in individual cribs, although at times, because of over-crowding, two infants temporarily occupy the same crib. In such instances, the heads of the two babies are placed at opposite ends of the bed.

A child is bathed on alternate days. Except when being bathed, the younger children spend practically their entire time in their cribs. They are given milk and other liquids while lying in bed, the bottle being supported by a small pillow. When semi-solid foods are introduced, infants are sometimes held by an attendant and sometimes fed in bed. The children are placed in bed in the supine position. They are never placed prone and seldom get themselves into this position.

The paucity of handling is due primarily to the attendant-child ratio. On the average there were eight children per attendant. In addition to feeding the children, bathing them, and changing clothing and diapers, the attendants are also responsible for changing the bed-linen and cleaning the rooms, and have some responsibilities for preparing food. Each attendant is on duty 12 hours per day. In general there are 32 children and four attendants to a room, although this varies somewhat according to the size of the room. There is no assignment of attendants to particular children. The attendants have no special training for their work and are poorly paid. The emphasis on the part of the supervisors seems to be on neatness in the appearance of the rooms, with little attention to behavioral development.

In his crib the child is not propped up, and is given no toys. The child who can pull himself to sitting, and hence is in some danger of falling from his shallow crib, is placed, when awake, on a piece of linoleum on the composition stone floor of the room. Until he himself achieves the sitting position he remains in bed. In two rooms some of the children who can sit are seated in a row on a bench which has a bar across the front to prevent falling. Aside from these two benches and the frames for the cribs, the rooms have no children's furniture and no play equipment of any kind.

2. *Institution II*

This institution accepts children over three years of age. The children in this institution come mainly from Institution I. Child care practices in II are a continuation of the practices existing in I, but sanitation and cleanliness are poorer and the appearance of the children suggests that nutrition and health are poorer. However, in neither I nor II are there any records of growth in height or weight, and it was not possible for us to obtain any objective assessment of nutritional status.

3. *Institution III*

Institution III was established only one year prior to the present study. It was started primarily to demonstrate improved methods of institutional care. The children in III come from Institution I but are selected for transfer in the early months of life. It seems likely that those sent to Institution III are chosen from among the more retarded children. They remain in III until three years of age unless adopted before that date. The number of children per attendant is 3–4. Children are held in arms while being fed, are regularly placed prone during part of the time they are in their cribs, are propped up in a sitting position in their cribs at times and are placed in play pens on the floor daily when above four months of age. Numerous toys are provided. Attendants are coached in methods of child care, and supervisors emphasize behavioral development as well as nutrition and health.

Individual growth charts are available for each child in Institution III and show without exception that these children are much below prevailing weight norms on arrival but attain normal weight within a few months.

C. Types of Behavioral Data

Quantitative observations on the behavioral status of the groups described above were made only with regard to motor coördinations. Some general observations on social and emotional behavior will be presented after motor behavior has been discussed.

In respect to motor development, each child who was a subject of this study was classified with regard to his ability to meet each of the following behavioral criteria:

1. *Sit alone.* The child was placed in a sitting position on the floor. He was scored as sitting alone if he maintained this position for one minute. However, if a child could maintain this position at all he ordinarily could maintain it indefinitely.

2. *Creep or Scoot.* The child was placed sitting on the floor and was encouraged to locomote by having the attendant hold a cookie, or extend her arms, toward the child at a distance of about six feet. He was scored as passing the test if he covered the distance in any manner. If he locomoted, his mode of progression was recorded. The modes of locomotion will be discussed at a later point.

3. *Stand by holding.* The child was held under the arms and placed adjacent to the horizontal bars of a child's bed. It was observed whether or not he grasped the bars and maintained a standing position.

4. *Walk by holding.* The child who could stand by holding was observed for some minutes to determine whether he took steps while holding. He was urged to walk by the attendant.

5. *Walk alone.* The child who could walk by holding objects was placed standing without support and was encouraged to walk to the outstretched arms of the attendant. The child was scored as walking alone if he took at least two steps without support.

In the above tests one of the attendants with whom the child was familiar was coached to make the tests while the experimenter remained at a distance of six feet or more from the child and somewhat behind him. This procedure was followed because it was found that the child's unfamiliarity with the experimenter often inhibited the child's behavior if he was tested by the examiner himself. Communication between the attendant and the examiner was conducted via an Iranian interpreter. Tests were conducted among the children of a given room only after the experimenter and the interpreter had made several visits to the room and somewhat decreased the children's shyness. If a child failed a test, the attendant was asked whether or not he could usually perform the required response. If the answer was positive, renewed efforts were made to elicit a successful performance. The experimenter is convinced that subjects who were scored as failing a test were actually unable to perform the required task.

The numbers of children tested at each age level in each institution are shown in Table 1. In Institutions I and II the total number of children tested was 123. In selecting children to provide this sample, the children of appropriate ages were selected at random from each of several rooms, the rooms so far as we could determine not being unusual in any respect. However, we excluded from testing any child who had sensory or motor defects, who was ill or who had recently been ill. In Institution III all children between age one and three were tested. They totaled 51.

TABLE 1
PER CENT OF EACH GROUP PASSING EACH TEST

Institutions	I	I	II	III	III
N	50	40	33	20	31
Ages	1.0-1.9	2.0-2.9	3.0-3.9	1.0-1.9	2.0-2.9
Sit alone	42	95	97	90	100
Creep or Scoot	14	75	97	75	100
Stand holding	4	45	90	70	100
Walk holding	2	40	63	60	100
Walk alone	0	8	15	15	94

D. RESULTS OF TESTS

Table 1 shows the per cent of each group which passed each test. The reader is asked to direct his attention first to the retardation which is evident in Institutions I and II. Among those children in Institution I who were between 1.0–1.9 years of age, fewer than half could sit alone and none could walk alone. In normative studies, of home-reared children, such as those conducted by Jones (6), Gesell (5), Dennis and Dennis (2) and others, it has been found that by nine months of age all normal non-institutional American children can sit alone. By two years of age nearly all can walk alone. A majority of the children of Institution I cannot perform these responses at ages at which almost all home-reared children can perform them. It will be noted that even between 2.0–2.9 years of age only 8 per cent of the children in Institution I are able to walk alone and only 15 per cent of those children in Institution II who are 3.0–3.9 years of age are able to walk alone. We are not aware that any groups so retarded as Groups I and II have previously been reported.

In Institution III the picture is different. Of those children between 2.0–2.9 years of age nearly every child is able to walk unaided. While these children do not equal the performance of home-reared children, their motor behavior is much superior to that of children in Institutions I and II. In other words it is not institutionalization per se which handicaps Groups I and II since Group III children who are also institutionalized are but slightly retarded in motor development. The records of Group III also show that motor retardation is not a general characteristic of Tehran children.

Of special note is the difference in types of pre-walking locomotion between Institutions I and II on the one hand and Institution III on the other.

Of the 67 children in Institutions I and II who engaged in creeping or scooting, only 10 did so by creeping, i.e., going on hands and knees or on hands and feet. All others progressed by "scooting," i.e., their locomotion took place in the sitting position, the body being propelled forward by pushing with the arms aided by propulsion from the legs. Many children who could not walk were quite adept at scooting.

Since tests for creeping or scooting were made when the child was in a sitting position, it might seem that the frequency of scooting was due to the nature of the starting position. To test the effect of starting position, many subjects who were "scooters" were placed prone and offered a cookie at some distance, a powerful incentive for locomotion in these children. In each case the child first pushed himself to a sitting position and then scooted. Scooting was definitely the preferred mode of locomotion even when the child was placed prone. So far as we could determine, the majority of the scooters were completely unfamiliar with creeping.

In Institution III, the reverse situation prevailed. Of 15 children who were observed to creep or scoot, all progressed by creeping. No scooting whatsoever was seen in this institution, yet tests were made from the sitting position as with Groups I and II. When placed sitting and encouraged to locomote, the children leaned forward, got themselves on hands and knees, and crept.

E. Interpretative Comments on Motor Development

Let us examine now the probable reasons why the children in Institutions I and II were so severely retarded relative to home-reared children and why they were so much more retarded than children in Institution III. Several different possibilities need to be considered.

Attention should first be directed to malnutrition as a possible cause of retarded motor development. As noted earlier there can be no doubt that many of the children in Group I were much smaller and lighter than non-institutional children and children of the same age in Group III. There can be no doubt, too that malnutrition can be so severe as to interfere with motor performance and motor progress. But the question at stake is not whether malnutrition can affect motor functions but whether malnutrition was in fact a major cause of the retardation of Groups I and II.

We are inclined to think that undernourishment was not the major factor. The following considerations have led us to this interpretation: In the first place, Groups I and II were not entirely listless and inactive. In this connection we need to bring out a fact that we have not noted in earlier sections, namely that these children engaged to a considerable extent in automatisms such as head shaking and rocking back and forth. In many cases, these actions were quite vigorous. These activities tend to indicate that these children were not slow in motor development simply because of motor weakness.

The second consideration is somewhat similar to the first, namely, that the locomotor activities in which the children in Groups I and II engaged seem to require as much as or more energy than the locomotor activities which are usual at their respective ages, but in which they did not engage. For example, while few two-year-olds in Group I walked, three-fourths of them locomoted, chiefly by scooting. No physiological data are available, but it seems likely that the metabolic cost of covering a certain distance by scooting is as great as, or even greater than, the effort required to go the same distance by walking. Certainly this would be true for an adult, but of course one cannot argue from the adult to the child. At any rate the possibility exists that the reason that these children scooted was because this was the only form of locomotor skill which they had learned, not that they were too weak to walk.

This interpretation seems to be borne out by the fact that the pre-walking methods of locomotion were different in different groups. The retarded groups scooted. It is difficult to believe that malnutrition can lead to scooting rather than creeping. It is far from obvious that scooting is "easier" than creeping. If it is, why should not all children choose the easier method? In other words, the differences between groups seem to us to be due to the outcome of different learning situations rather than to differences in nutritional status.

What were the differences in the situations faced by Groups I and II and Group III which may account for the development of two different types of locomotion and different degrees of retardation? We suggest the following:

In Group III and in many homes infants are propped up in a sitting position, or held in a sitting position. In this position the child can raise his head and can partially raise his shoulders for short periods and can relax these efforts without falling. He can thus practice some elements of sitting. On the other hand, the child who remains on his back has no such opportunities to learn to sit. In some respects it is surprising that children who are never propped up or held on the lap are able to learn to sit at all. But it will be remembered that in Groups I and II some children could not sit until they were more than two years of age. Until they could sit alone, all forms of locomotion were impossible for them, because they were not placed in a position in which creeping was possible.

This is not true in Group III. In this group and in many homes, the child is frequently placed prone in bed or on the floor. In this position he can raise his head from the surface, push with his arms, raise his chest, pull his arms and legs beneath his body—in other words, he can practice acts which are employed in creeping. The child who lies on his back nearly every moment of the day is denied such practice. Thus one specific item of child care, i.e., occasionally placing the child face downward, may well contribute to the development of creeping in most children and its absence may account for the lack of creeping in Groups I and II.

The question may be raised as to why children in Institutions I and II did not get themselves into the prone position in their cribs. Repeated observations of these infants in their cribs showed that few ever attained the prone position. The probable reasons are the small size of the cribs and the softness of the beds, both of which made turning over very difficult.

It is likely that this item, i.e., absence of placement in the prone position, may lead to delayed development not only in regard to creeping but also in respect to walking. The child who can creep can go to a piece of furniture, grasp it and pull to his knees. This may lead to walking on his knees while holding furniture. Many children go from knee walking to walking by holding to furniture and thence to walking alone. In contrast to the child who creeps, the child who scoots to a piece of furniture is sitting when he arrives at his goal and can attain a higher position only by lifting his entire weight by his arms. In our opinion, the lack of creeping accounts in large measure for the retardation in walking of Groups I and II.

We are well-aware that some persons have interpreted the behavioral retardation of institutional infants to emotional factors rather than to a paucity of learning opportunities. Some have even suggested that under certain conditions institutional infants simply "waste away" from psychological, not from medical causes, a process called marasmus.

If marasmus actually exists, it has somehow been escaped by several hundred children in Iranian institutions living under conditions which are supposed to foster it. Although the prevailing emotional tone of children in Institutions I and II is dysphoric, it is difficult to conceive of mechanisms whereby their unhappiness retards their motor development and causes them to scoot rather than to creep.

There remains the necessity of relating the results of the present study to certain findings reported earlier by the present author. We refer to a study which found no apparent effect of cradling upon the motor development of Hopi children (3) and a study which indicated that infant development can proceed normally under conditions described as "minimal social stimulation" (1). On the surface these results seem contradictory to those here reported, because the former studies found that environmental deprivations had but little effect whereas the present study reports that major consequences can ensue from them. In fact, however, the studies are not contradictory but complementary. To bring the results of these studies into harmony, one needs only to examine the kinds of deprivation which were involved and their severity. Certain differences among these studies seem to us to be crucial. The Hopi children were limited in regard to learning opportunities only *while on the cradleboard.* As we pointed out in our original report, they were on the cradleboard chiefly during these sleeping hours, when in any case little learning is expected to occur. When awake they were handled, held upright against the mother, placed sitting on her lap, and placed prone. Their deprivation of learning opportunities was much less than that encountered by the children in Institution I who 24 hours per day for many months remained in a supine position.

A similar contrast exists between Rey and Del, the subjects of an experiment in environmental deprivation, and children in Institutions I and II. Rey and Del were not deprived to the same degree nor in the same manner as the institutional children described above. As the original report shows (1), Del and Rey, beginning at nine months, were regularly placed in a prone position on a pad on the floor. After it was found that they could not sit alone they were given special practice in sitting. Del and Rey were also given special training in supporting their weight when held upright. Such training was not given in Institutions I and II.

These experiences with special training given to Del and Rey suggest that the retardation of the institutional children could be fairly rapidly remedied if intensive specialized practice were given them. Unfortunately it was not possible for us to undertake such experiments while we were in Tehran. The speed with which delayed skills can be developed remains an important problem for future researches with institutional children.

So far as the permanency of motor deficiencies is concerned it should be noted that Institution II had many children between ages 6 and 15 years who presumably were as retarded at ages two and three as were the children whose behavior was described above. Yet these children were attending school, playing games, doing chores, and being trained in difficult skills, such as the weaving of Persian rugs. There was nothing in their general behavior to suggest that any permanent consequences issued from the extreme retardation in motor development during the early years. To be sure, we have no direct evidence that these children were retarded at two and three years of age, but so far as we could ascertain there has been no change in the child care offered by Institutions I and II and no reason to suppose that their early development was different from that of their counterparts in the present study.

Finally let us note that the results of the present study challenge the widely-held view that motor development consists in the emergence of a behavioral sequence based primarily upon maturation. Shirley's chart of the motor sequence is a textbook favorite. It shows sitting alone at seven months, creeping at 10 months, and walking alone at 15 months. The present study shows that these norms are met only under favorable environmental conditions. Among the children of Institution I not only was sitting alone greatly retarded but in many cases creeping did not occur. Instead, an alternate form of locomotion was employed. These facts seem to indicate clearly that experience affects not only the ages at which motor items appear but also their very form. No doubt the maturation of certain structures, which as yet cannot be identified, is necessary before certain responses can be learned, but learning also is necessary. Maturation alone is insufficient to bring about most post-natal developments in behavior. This is also the conclusion which we reached in the Del-Rey experiment, but the present study supports this position more dramatically because the limitations of learning in Institutions I and II are more drastic and more long-continued than were those in the Del-Rey study.

F. SOCIAL AND EMOTIONAL BEHAVIOR

Only incidental observations were made relative to social and emotional behavior. Several of these had to do with the infants' reactions to visitors.

In the weeks preceding our tests, it appears that Institution I seldom had visitors. The children of Institution II formerly had few visitors but several weeks before our arrival a volunteer social service group, aware of the isolation of these children, began to make periodic visits to them, taking them from their beds, holding them, and carrying them about. Institution III also had several visitors, partly because of the demonstration nature of this orphanage.

Children in Institution I, probably because of their unfamiliarity with visitors, were somewhat afraid of us during our first visit. They did not smile with us and, in most cases, would cry if we picked them up. On repeated visits, however, they became more friendly, smiled at us, and before our work was completed some of them would hold out their arms to be carried.

Most of the children in Institution II were positive to visitors at the beginning of our work. Several employed attention-seeking devices before visitors and cried if other children were selected for attention. In contrast in Group III, probably because of the greater time spent with attendants and because of their familiarity with visitors, there was little fear of strangers and only limited attention seeking.

Eagerness for food appeared to be greatest in Institution II. In this institution there was much crying before meal time. Children of this group handled cups and spoons quite well. In general there was very little wasting of food on the part of these children. Cups of milk were reached for eagerly, handled carefully, and drunk rapidly. There were attempts, sometimes successful, on the part of those who had finished eating to obtain the food of others, and hitting, pinching, and biting were sometimes the outcomes of such clashes. Children who could not walk could nevertheless manage to attack others and to defend themselves with considerable skill. After feeding they became much more jovial and nearly every child could be made to smile or laugh by an adult who shook him lightly or tickled him.

G. SUMMARY

This paper has presented data concerning behavioral development among 174 children, aged one year to four years, in three Iranian institutions. In Institutions I and II infant development was greatly retarded. The behavioral progress of children in the third institution was much less retarded. The interpretations offered for these differences in behavior among the children of different institutions are as follows: the extreme retardation in Institutions I and II was probably due to the paucity of handling, including the failure of attendants to place the children in the sitting position and the prone position. The absence of experience in these positions is believed to have retarded the children in regard to sitting alone and also in regard to the onset of locomotion. The lack of experience in the prone positions seems in most cases to have prevented children from learning to creep; instead of creeping, the majority of the children in Institutions I and II, prior to walking, locomoted by scooting. In Institution III, in which children were frequently handled, propped in the sitting position, and placed prone, motor development resembled that of most home-reared children. The retardation of subjects in Institutions I and II is believed to be due to the restriction of specific kinds of learning opportunities. This interpretation was found to be congruent with the results of other studies in environmental deprivation. In the light of these findings, the explanation of retardation as being due primarily to emotional factors is believed to be untenable. The data here reported also show that behavioral development cannot be fully accounted for in terms of the maturation hypothesis. The important contributions of experience to the development of infant behavior must be acknowledged.

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RELATIONSHIPS BETWEEN SOCIOECONOMIC VARIABLES AND THE BEHAVIOR OF MOTHERS TOWARD YOUNG CHILDREN*

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A. Problem

Several recent studies of maternal childrearing attitudes and practices have indicated that there is a relationship between the nature of these practices and socioeconomic status (1, 5, 6, 8, 9, 10, 14, 15). If this is true, then it becomes necessary to take socioeconomic factors into account when evaluating the effects of maternal behaviors on children's personality development. For example, Sears, Maccoby, and Levin (14) have found, on the basis of interviews with mothers of five-year-olds, that "the middle-class mothers were generally more permissive and less punitive toward their young children than were working-class mothers," and that there was generally more warmth expressed by the middle-class (higher socioeconomic status) mothers in dealing with their children. Another frequent finding is that parents treat young boys and girls differently from early infancy. Several studies indicate that mothers express greater warmth toward girl babies, but permit more aggression in boys. As for specific techniques of training, boys usually get more physical punishment and girls are more often controlled by the giving or withholding of approval or love. Hubert and Britton (6) suggest a tendency toward parental identification with the infant of the same sex. Sears, Whiting, *et al.* (15) suggest that mothers feel more assured with their daughters and freer to control them.

So far, with the exception of the studies of Baldwin, *et al.* (1), the reported findings on these parent and child relations have been based on data from interviews and questionnaires. It has seemed worthwhile, therefore, to report another array of similar relationships found when scores are based on observed maternal behavior and attitudes rather than on direct questions about their practices.

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B. Description of the Variables

The maternal variables used here were developed in order to study parent-child relationships in the subjects of the Berkeley Growth Study (7). For this purpose two sets of rating scales were devised. The first, (Form I: Quantification of Behavior Observations, composed of 32 scales) was based on descriptive protocols of the behavior of 56 mothers as observed in the testing periods in 1928-32, during the first three years of the children's lives. The scales were then adapted (into Form II: Quantification of Interview Data, with 28 scales) and applied to the records of interviews of a subsample of 34 of the mothers. These interviews were made in 1939-42 by a psychologist (Pearl Bretnall Meissner). They were not directly concerned with maternal attitudes, but were directed toward characterizing those aspects of the home environment that were relevant to the child's intellectual development.

The scores derived from these ratings, described by Schaefer, Bell, and Bayley (13), have yielded essentially two orthogonal factors that may be simply described as autonomy for the child vs. control, and loving-acceptance vs. hostile-rejection. These factors were found to be essentially the same for both scales. Furthermore, within each form the maternal category ratings are found to be correlated with each other in such a way that they can be arranged in a Guttman "circumplex" (4, 10). Nine of the behavior categories that are common to both forms were selected for use in our comparative studies. These nine have the heaviest loadings in the two factors, and also are generally representative of the range of behaviors observed. They are shown in their circumplex positions in Figure 1. Their intercorrelations (10), reliabilities (13), and consistencies over time (this last on 31 cases) (12) are summarized in Table 1. As the score on each category is a composite of ratings on 4 to 8 relevant "behaviors," we find that by combining the ratings of three judges the category scores are adequately reliable.

These observations were made on a primarily middle-class population living in a metropolitan area near a large University. At the time of the infant observations the pediatric and other "expert" advice given to parents was oriented toward insuring adequate nutrition and optimal physical growth. Rigid schedules of care and feeding were recommended, and parents were advised to avoid overstimulating their infants. Although the better-educated, higher-status mothers were more likely to hear and read this advice, it is very likely that at that time most mothers were less concerned than they are now to seek professional advice.

The socioeconomic variables of the families studied have been described by Bayley and Jones (3) and Bayley (2). They consist of: number of years education of the mother and of the father; the father's occupation (rated on the Taussig scale); family income; and a social rating based on the home visitor's ratings of several aspects of the home and neighborhood. These separate scales were also combined into a composite socioeconomic scale. The data were obtained by a home visit soon after the birth of the child (1928-29), and again in 1938.[2] There is an additional socio-cultural score, based on ratings of the home and family, made in 1939-42 by the home interviewer. Although the average scores are relatively high, as of 1929 the families range from unskilled labor, relief, and three years education to professional, $10,000 income, and doctoral degrees, from bare-subsistence shacks to beautiful servant-operated homes in superior neighborhoods. Sixty-eight per cent were classified as "Professional or executive," and "White

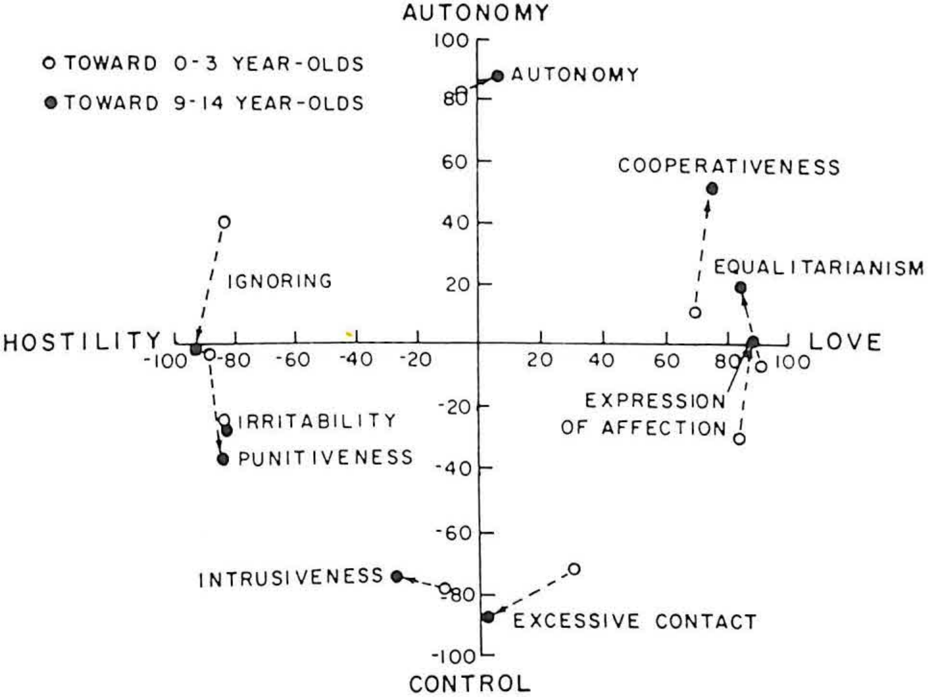

FIGURE 1
CIRCUMPLEX OF MATERNAL BEHAVIOR FOR NINE SELECTED ITEMS RATED BOTH AT 0-3 YEARS AND 9-14 YEARS

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[2] The first home visits were made by a social worker, Miss Frances M. Welch; the 1938 visits were made by a psychologist, Mrs. Jean Carter.

TABLE 1

CORRELATIONS FOR RELIABILITY, INTERRELATIONS, AND CONSISTENCY OF MATERNAL BEHAVIOR

		First-Three-Year-Behavior Observations								
9-14 Year Interviews	Reli-abilities*	1 .80	2 .85	3 .84	4 .83	5 .67	6 .76	7 .82	8 .78	9 .89
1. Grants Autonomy	.77	*.26*	.22	—.06	—.30	—.80	—.64	—.12	—.03	.36
2. Cooperative	.91	.46	*.69*	.65	.56	.11	—.23	—.69	—.62	—.63
3. Equalitarian	.83	.24	.70	*.56*	.82	.28	—.04	—.75	—.78	—.76
4. Expresses Affection	.78	.12	.65	.85	*.58*	.48	.16	—.56	—.73	—.80
5. Excessive Contact	.81	—.76	—.46	—.15	.06	*.24*	.52	—.10	—.22	—.49
6. Intrusive	.77	—.67	—.67	—.37	—.22	.64	*.34*	.35	.08	—.20
7. Irritable	.89	—.26	—.80	—.72	—.69	.27	.58	*.64*	.76	.61
8. Punitive	.86	—.30	—.88	—.78	—.74	.36	.59	.89	*.43*	.67
9. Ignoring	.89	—.05	—.76	—.79	—.82	.02	.34	.74	.81	*.62*

Center diagonal: Consistency, or inter-scale *r*'s, $N = 31$.
Upper right: Intercorrelations of Maternal Behavior Ratings (0-3 years) $N = 56$.
Lower left: Intercorrelations of Maternal Behavior Ratings (9-14 years) $N = 34$.
* Reliabilities, average of interjudge agreements between three judges, adjusted by Spearman-Brown formula.

Collar" on the Taussig scale. The intercorrelations of these socioeconomic variables have been reported (2), and are repeated here in Table 2 for the two sets of data. In 1929 the mother's education seemed relatively independent of the other scores; but in general the intercorrelations are fairly high and average about .60.

What then are the relationships of these socioeconomic (*S-E*) variables to the characteristic behaviors of the mothers toward their infants? The judges who rated the maternal behavior did not know the mothers, and knew nothing of the socioeconomic status except as it might be deduced from the descriptive protocols that had been written at the times of the infant tests and the 10-year interviews.

TABLE 2
CORRELATIONS BETWEEN FIVE SOCIOECONOMIC FACTORS 1938
(Figures in parentheses refer to data for 1928-1929)
N(1928-29) = 59. N(1938) = 47.

		Correlations			
	Means	Fathers' education	Occupation	Income	Composite social rating
Mothers' education	12.9 years	.56 (.56)	.51 (.57)	.57 (.24)	.48 (.37)
Fathers' education	13.7 years		.65 (.74)	.65 (.74)	.58 (.57)
Occupation	White Collar			.72 (.70)	.68 (.69)
Family income	$2,845				.74 (.82)

C. RESULTS

The intercorrelations of scores for maternal behavior, Form I, (0–3 years, 1928–32) and Form II (9–14 years, 1939–42) with the 1938 socioeconomic variables are given in Tables 3 and 4 for the 31 mothers on whom data are available at both age levels. The maternal variables have been arranged in the order in which they fall on the circumplex, moving clockwise, and thus adjacent traits are positively, and distant traits negatively, correlated (Figure 1). Relatively few of the r's between the socioeconomic and maternal behavior variables are statistically significant. However, they do display an interesting and orderly trend in the size and direction of correlation. This trend shows up clearly when the r's are charted, as in Figures 2 and 3. All of the *S-E* variables tend, at both ages, to be correlated positively with traits in the upper and right segments of the circumplex. These are Granting Autonomy, Coöperation, Equalitarian Treatment, and, in lesser degree, Expressing Affection. On the other hand, they are negatively correlated with the traits in the lower and left segments of the circumplex—Excessive Contact, Intrusiveness, Irritability, and Punitiveness. They are

TABLE 3

CORRELATIONS BETWEEN MATERNAL BEHAVIOR RATINGS (0-3 YEARS) AND SOCIOECONOMIC DATA (1938) FOR 31 CASES AVAILABLE AT BOTH AGE LEVELS

Socioeconomic variables	Maternal behaviors 1 Grants Autonomy	 2 Cooperative	 3 Equalitarian	 4 Expr. Affection	 5 Exces. Contact	 6 Intrusive	 7 Irritable	 8 Punitive	 9 Ignoring
Fathers' education	.15	.35	.17	—.01	—.09	—.26	—.20	—.05	—.11
Mothers' education	.44*	.01	—.07	—.32	—.35	—.20	.13	.09	.21
Income	.18	.11	.07	—.04	—.25	—.21	—.13	—.12	—.12
Fathers' occupation	.21	.22	.10	—.12	—.21	—.17	—.14	—.07	—.11
Total social	.33	.15	.10	—.03	—.32	—.20	—.24	—.28	—.07
Socioeconomic	.28	.19	.13	—.06	—.28	—.26	—.18	—.19	—.10
Socio-cultural**	—.01	.10	.21	.07	.02	.07	—.10	—.06	—.19

* *r*'s of .36 or higher are significant at the .05 level or better.

**Composite of ratings by P. Bretnall made in 1938-42. Concordance of first 5 *S-E* variables $W' = .706$. $r_{s\,av} = .63$. Significant at .001 level of confidence.

TABLE 4

CORRELATIONS BETWEEN MATERNAL INTERVIEW RATINGS (9-14 YEARS) AND SOCIOECONOMIC DATA (1938) FOR 31 CASES AVAILABLE AT BOTH AGE LEVELS

Socioeconomic variables	Ratings from maternal interviews 1 Grants Autonomy	2 Cooperative	3 Equalitarian	4 Expr. Affection	5 Exces. Contact	6 Intrusive	7 Irritable	8 Punitive	9 Ignoring
Fathers' education	.22	.39*	.21	.13	—.30	—.28	—.29	—.35	—.22
Mothers' education	.38*	.21	.17	.01	—.48**	—.25	.01	—.10	—.04
Income	.09	.03	.08	—.07	—.25	.09	.09	—.04	.03
Fathers' occupation	.28	.28	.17	.13	—.30	—.13	—.12	—.19	—.16
Total social	.22	.14	.24	.11	—.17	.07	—.11	—.14	—.07
Socioeconomic	.18	.20	.21	.07	—.29	—.01	—.06	—.16	—.10
Socio-cultural***	.36*	.38*	.41*	.29	—.48**	—.25	—.19	—.36*	—.30

* *r*'s of .36 or higher are significant at the .05 level of confidence.

** *r*'s of .47 or higher are significant at the .01 level of confidence.

*** Composite of ratings by P. Bretnall, made in 1938-42. Concordance of first 5 *S-E* variables $W = .746$. $r_{s\ av} = .68$. Significant at .001 level.

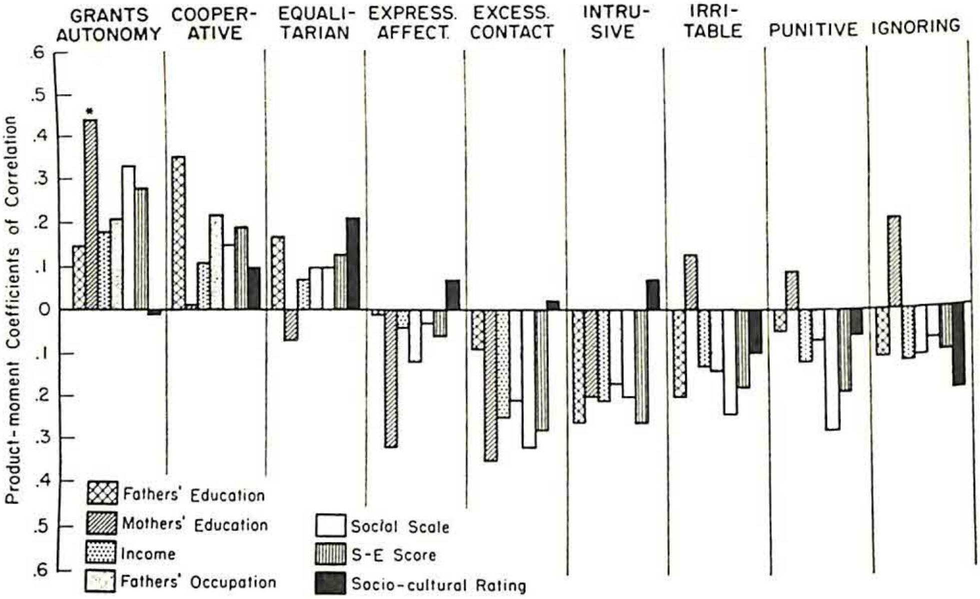

FIGURE 2

Diagram of Correlations Between Socioeconomic Status (1938) and Behavior of Mothers Toward Their Infants Under Three Years
(*Significant at the .05 level of confidence.)

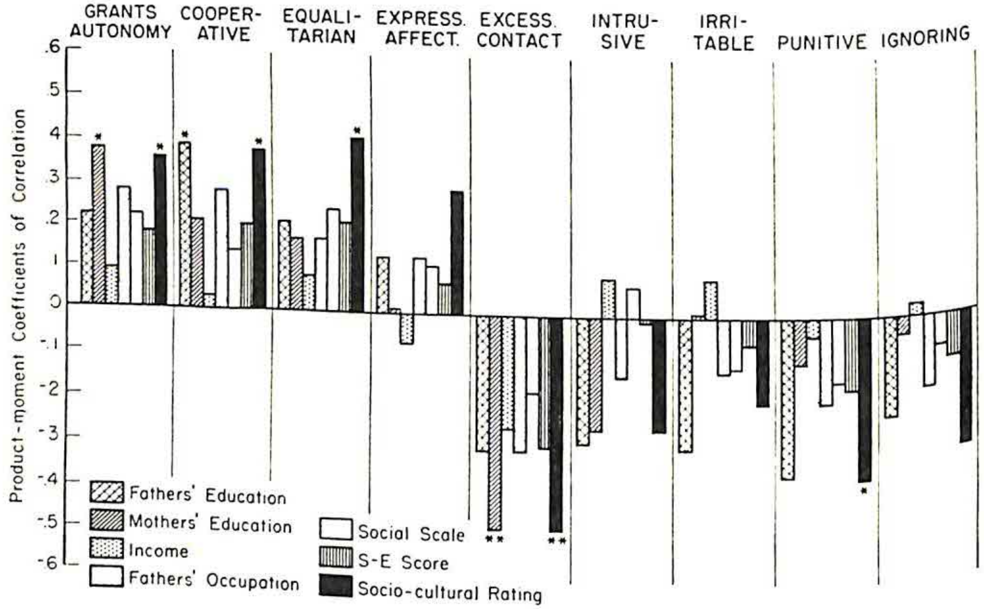

FIGURE 3

Diagram of Correlations Between Socioeconomic Status (1938) and Attitudes of Mothers Toward Their 9-14 Year Old Children
(*Significant at the .05 level of confidence.)
(**Significant at the .01 level of confidence.)

also negatively correlated to some extent with Ignoring. The positive *r*'s that reach or approach significance are all within the first three behavior categories in the tables (Autonomy, Coöperation, and Equalitarian Treatment). The significantly negative *r*'s are found among those for Excessive Contact and Punitiveness. These relations are more evident in the attitudes displayed toward the older children than toward the infants.

To test further the significance of these trends in the pattern of relationships Kendall's coefficient of concordance (16) was computed for the rank order of size of *r* between the maternal behavior traits and the several measures of socioeconomic status. In both of these tables the *W* (coefficient of concordance) is significant. It is true, however, that a test of significance may not be applicable here, because the socioeconomic variables are intercorrelated about .60, indicating that they are all roughly similar measures of socioeconomic status, although different aspects of it. Nevertheless, the basic relation between *S-E* status and maternal behavior appears to be borne out by the fact that even though the correlations are low, the *pattern* of the relations is very consistent. The *W* for the pattern of behavior of mothers toward infants is .71 ($r_{s\,av} = .63$); for the mothers' expressed attitudes toward their 10-year-olds, the *W* is .75 ($r_{s\,av} = .68$). When correlations are computed using the 1929 *S-E* variables they are essentially the same: these coefficients of concordance are: for the 0–3 year Form I maternal scores .60 ($r_{s\,av} = .50$), and for the 9–14 year Form II maternal scores .86 ($r_{s\,av} = .82$).

Although the relationships are very moderate, these findings are in the direction of agreement with other studies that indicate warmer, more accepting, and permissive attitudes among the higher *S-E* status mothers, and more dominating and punitive attitudes among the lower status mothers. Our data, taken together with these other reports, point toward some consistency of the socioeconomic pattern of maternal attitudes, from observed behavior to interview and from infants to older children.

In another grouping of the maternal behavior ratings (0–3 years), correlations with socioeconomic scores have been computed separately for the mothers of boys and of girls. This was done because it has seemed possible that differences in the treatment of boy babies and girl babies could obscure the socioeconomic variables. An analysis of variance shows no difference in *S-E* scores between the mothers of boys and girls, or of first-born and later-born children in the Berkeley Growth Study. Therefore, any relationships we find cannot be attributed to sampling differences in these respects.

The means and *SD*'s of the scores on Maternal Behavior Form I (0–3 years) are given in Table 5 for the mothers of boys and girls separately. Although they are not significant, such sex differences as there are point toward a slightly more autonomous, coöperative, equalitarian treatment of daughters and more irritable, punitive, ignoring treatment of sons. Such differences would be in the same direction as reported in other studies (6, 9, 14).

TABLE 5

MEANS AND *SD*'S OF THE MATERNAL BEHAVIOR RATINGS (FORM I, 0-3 YEARS) FOR THE MOTHERS OF BOYS AND OF GIRLS, SEPARATELY

Behavior trait	Boys $N = 26$ Mean	Boys $N = 26$ *SD*	Girls $N = 27$ Mean	Girls $N = 27$ *SD*
Grants autonomy	15.15	5.55	16.19	4.70
Cooperative	13.46	4.67	14.93	4.69
Equalitarian treatment	13.85	4.16	15.11	5.47
Expresses affection	14.23	4.52	14.59	5.31
Excessive contact	9.85	3.21	9.96	3.51
Intrusive	15.08	4.94	14.41	4.75
Irritable	16.35	5.12	14.41	4.67
Punitive	10.81	3.52	9.70	3.26
Ignoring	15.92	4.80	14.78	5.31

The correlation tables (Tables 6 and 7) for the mothers of boys again exhibit an orderly pattern of relationship between the *S-E* variables and the maternal behavior scores when the latter are arranged clockwise around the circumplex. Although with so small a sample only a few of the coefficients are significant, their consistency and directional trends are impressive. This is most evident for the 1929 *S-E* variables, for which we have both more cases and higher correlations. Similarity of patterning in the *r*'s is indicated for Table 6 by a *W* of .90 ($r_{s\ av} = .85$) and for the 1938 *S-E* data in Table 7 by a *W* of .725 ($r_{s\ av} = .66$).

The relationships for the mothers of girls, however, are much less clear (Tables 8 and 9). None of these *r*'s is large enough to be significant but in spite of this the concordance (*W*) for the 1929 *S-E* scores (Table 8) is .919, ($r_{s\ av} = .88$). However, the pattern breaks down in Table 9, which deals with the low correlations of the mother-daughter Form I (0–3 year) scores and the 1938 *S-E* scores: *W* is .24 ($r_{s\ av} = .05$).

If we run a coefficient of concordance for the *r*'s between Form I scores for mothers of boys and girls separately with the total *S-E* Scale scores for 1929 and 1938 (four series of *r*'s from Tables 6, 7, 8, and 9) we obtain a *W* of .79 ($r_{s\ av} = .72$). The later (Form II) maternal scores, when correlated with the same four variables show similarly high concordance: *W* is .83 ($r_{s\ av} = .77$). The general concordance between the two independent

TABLE 6

Correlations Between Maternal Behavior Ratings (0-3 Years) and 1929 Socioeconomic Data for 26 Boys

	Maternal behaviors								
Socioeconomic variables	1 Grants Autonomy	2 Cooperative	3 Equalitarian	4 Expr. Affection	5 Exces. Contact	6 Intrusive	7 Irritable	8 Punitive	9 Ignoring
Income	.06	.30	.53**	.33	—.15	—.19	—.56**	—.60**	—.37
Fathers' occupation	—.02	.41*	.42*	.21	—.25	—.11	—.45*	—.35	—.50**
Total social	.11	.24	.34	.12	—.40*	—.13	—.35	—.37	—.34
Socioeconomic	.14	.40*	.50**	.23	—.35	—.22	—.52**	—.47*	—.45**

* Significant at the .05 level of confidence.

** Significant at the .01 level of confidence. $W = .900$ (level of confidence between .01 and .001). $r_{s\,av} = .85$.

TABLE 7

Correlations Between Maternal Behavior Ratings (0-3 Years) and 1938 Socioeconomic Data for 21 Boys

	Maternal behavior								
Socioeconomic variables	1 Grants Autonomy	2 Cooperative	3 Equalitarian	4 Expr. Affection	5 Exces. Contact	6 Intrusive	7 Irritable	8 Punitive	9 Ignoring
Fathers' education	.23	.52**	.36	.20	—.25	—.32	—.33	—.07	—.25
Mothers' education	.48*	.32	—.06	—.27	—.43*	—.27	—.12	.05	—.02
Income	.27	.22	.18	—.08	—.37	—.38	—.26	—.23	—.13
Occupation	.18	.34	.36	.14	—.37	—.22	—.42	—.30	—.38
Total social	.24	.28	.37	.13	—.33	—.22	—.43	—.48*	—.36
Socioeconomic	.32	.35	.36	.07	—.40	—.36	—.44*	—.40	—.32
Socio-cultural	.23	.11	.12	—.04	—.30	—.09	—.15	.00	—.13

* Significant at the .05 level.

** Significant at the .01 level. N is 26. Concordance of first 5 S-E variables. $W = .725$ ($p < .001$). $r_{s\,av} = .66$.

TABLE 8

CORRELATIONS BETWEEN MATERNAL BEHAVIOR RATINGS (0-3 YEARS) AND SOCIOECONOMIC DATA (1929) FOR 27 GIRLS

Socioeconomic variables	Maternal behavior 1 Grants Autonomy	2 Cooperative	3 Equalitarian	4 Expr. Affection	5 Exces. Contact	6 Intrusive	7 Irritable	8 Punitive	9 Ignoring
Income	—.26	—.05	.30	.23	.26	—.08	—.16	—.15	—.19
Fathers' occupation	—.12	.18	.25	.12	.21	—.10	—.23	—.27	—.18
Social	—.24	—.04	.34	.21	.31	—.07	—.16	—.20	—.21
Socioeconomic	—.16	.01	.31	.21	.23	—.11	—.17	—.25	—.17

Concordance of first 3 *S-E* variables. $W = .919$ (Signif. .005 and .001). $r_{s\ av} = .88$.

TABLE 9

CORRELATIONS BETWEEN MATERNAL BEHAVIOR RATINGS (0-3 YEARS) AND SOCIOECONOMIC DATA (1938): GIRLS

Socioeconomic variables	*N*	Maternal behavior 1 Grants Autonomy	2 Cooperative	3 Equalitarian	4 Expr. Affection	5 Exces. Contact	6 Intrusive	7 Irritable	8 Punitive	9 Ignoring
Father's educ.	27	—.07	.11	.24	.13	.13	—.22	—.22	—.21	—.15
Mothers' educ.	27	.21	.01	.03	—.07	—.17	—.23	.08	—.04	.17
Income	20	—.06	—.13	.13	.16	.06	—.07	—.01	.04	—.19
Occupation	22	.10	.03	—.18	—.13	.19	.06	.13	.15	—.06
Total social	20	—.02	—.04	—.02	.05	.03	—.11	—.09	—.15	—.03
Socioeconomic	24	—.01	.02	.23	.17	.04	—.18	—.10	—.17	—.12
Socio-cultural	22	—.16	.03	.25	.24	.21	.09	.03	—.06	—.20

Concordance of first 5 *S-E* variables. $W = .24$ not significant. $r_{s\ av} = .05$.

samples (mothers of boys and of girls) lends credence to the significance of the ordering of the correlations. It appears from a review of all the tables that the relationship between behavior and *S-E* status is clearer with the 1929 *S-E* data, which were obtained shortly after the children were born, and with the mother-son relationships. The over all tendency for concordance in the pattern of relations is shown in the eight sets of correlations with maternal scores (boys, girls, Form I, Form II, 1929 *S-E* score, and 1938 *S-E* score). The *W* for these is .66 ($r_{s\ av} = .61$).

Comparing mothers of boys with mothers of girls, we note that in certain of the early (Form I) maternal behavior variables there is at least consistency in the direction of sign in the correlation coefficients for the 1929, and 1938 *S-E* scores for the two sexes (compare Tables 6 with 8, and 7 with 9). This is true of the maternal categories: Ignoring the Child, and (with one or two exceptions) Punitiveness, Irritability, Intrusiveness, Expressing Affection, and Equalitarianism. However, there is a reversal of sign (between the sexes) for two categories: Grants Autonomy and Excessive Contact. These two categories are at opposite poles of the Freedom-Control factor, and may be thought of as approximating extremes of the same variable.

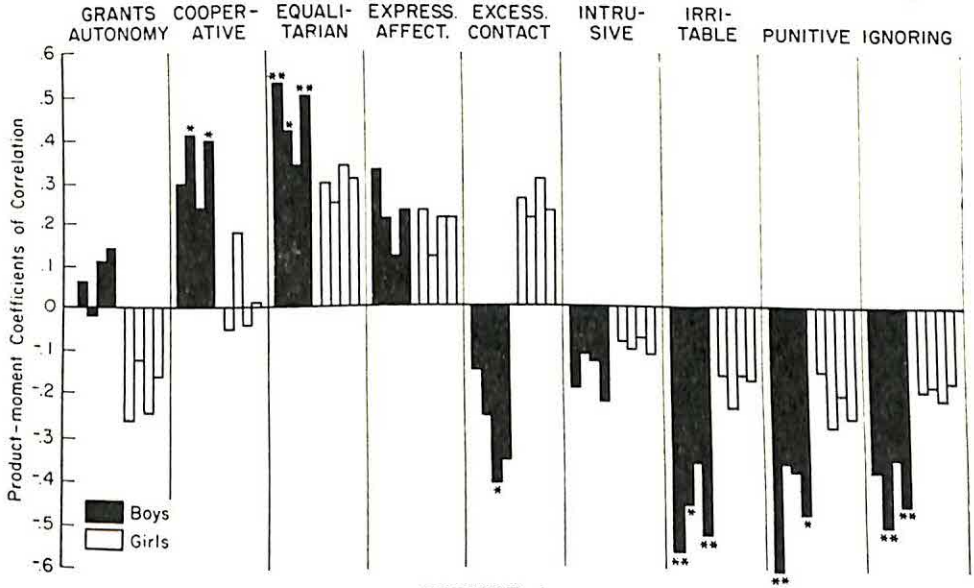

FIGURE 4

DIAGRAM OF CORRELATIONS BETWEEN SOCIOECONOMIC STATUS (1929) AND BEHAVIOR OF MOTHERS TOWARD SONS AND DAUGHTERS UNDER THREE YEARS: THE FIRST BAR IN EACH GROUP REPRESENTS INCOME; THE SECOND, FATHER'S OCCUPATION; THE THIRD, SOCIAL SCALE; AND THE FOURTH, *S-E* SCALE

(*Significant at the .05 level of confidence.)
(**Significant at the .01 level of confidence.)

The contrast is clearest for the 1929 *S-E* data but also evident in the 1938 *S-E* data. See Figure 4 and Tables 6, 7, 8, and 9. It would appear that our lower status mothers tend to have more closely controlling contact with their infant sons, but not with their daughters. Or conversely, that when their children are young the higher status mothers tend to grant their sons more autonomy but to control (perhaps more closely supervise) their daughters. This distinction, however, does not hold up for the 9 to 14 year olds. We see in Table 10 that though differences again are not significant and the number of cases is reduced, according to the scores on Form II the girls now seem to be given a little less freedom and their mothers are more punitive and irritable, when compared with the boys. Along with this general shift for the entire group the sex differences in treatment according to socioeconomic status have disappeared. This latter is shown in Table 11 for the

TABLE 10

MEANS AND *SD*'S OF THE MATERNAL INTERVIEW RATINGS (9-14 YEARS) FOR THE MOTHERS OF BOYS AND OF GIRLS, SEPARATELY

Behavior trait	Boys $N = 18$ Mean	*SD*	Girls $N = 16$ Mean	*SD*
Grants autonomy	15.17	5.10	14.14	4.68
Cooperative	15.83	4.69	13.81	5.74
Equalitarian treatment	15.22	4.60	14.31	5.74
Expresses affection	15.11	5.15	14.12	5.19
Excessive contact	14.94	4.89	14.94	4.62
Intrusive	15.11	4.78	15.25	5.23
Irritable	14.61	4.45	15.56	4.83
Punitive	13.94	4.26	16.25	5.18
Ignoring	14.72	4.99	15.75	1.03

TABLE 11

CORRELATIONS BETWEEN MATERNAL INTERVIEW RATINGS (9-14 YEARS) AND SOCIOECONOMIC SCALE SCORES FOR THE MOTHERS OF GIRLS AND BOYS SEPARATELY

Behavior trait	Boys ($N = 18$) 1929 *S-E*	1938 *S-E*	Girls ($N = 16$) 1929 *S-E*	1938 *S-E*
Grants autonomy	.40	.13	.22	.23
Cooperative	.32	.22	.15	.18
Equalitarian treatment	.35	.41	.20	.03
Expresses affection	.24	.21	.04	—.12
Excessive contact	—.41	—.24	—.22	—.37
Intrusive	—.25	—.00	—.01	—.04
Irritable	—.29	—.17	—.04	.08
Punitive	—.31	—.23	—.12	—.08
Ignoring	—.20	—.20	—.09	—.01

Concordance for the four sets of correlations: $W = .825$ (Signif. $< .001$). $r_{s\,av} = .767$.

total *S-E* Score. Now the higher *S-E* mothers of both boys and girls tend to grant more autonomy. Again the largest *r*'s are for boys; but none of the *r*'s is significant. The question arose whether the *r*'s might be influenced by elimination of a few cases at 9 to 14 years. But inspection of the two sets of scores of individual mothers of girls reveals a consistent shift that is in agreement with the change in correlation. These small differences may possibly point toward a complication of both cultural and age factors influencing the degree of autonomy allowed to girls.

D. Discussion

If we permit ourselves to generalize from the data on this small group, we might suggest that the social class differences in maternal behavior are in most respects more relevant to practices with boys than with girls. Other studies on parental behavior have not been presented in such a way as to permit corroborative evidence for this hypothesis. Those that report socioeconomic differences in childrearing do not discuss them as they relate to differential treatment of boys and girls. Where sex differences are compared, the socioeconomic variables have not been considered.

It is plausible to speculate that some of these sex differences will vary with social class and age of child in accord with class-linked differences in value systems. For example, our findings might serve to explain one set of discrepancies we have noted in the reports of two studies. In the predominantly professional-class families of the Hubert and Britton study (6) fewer boys' mothers than girls' mothers attempted toilet training during the first two years. The Sears, Maccoby, and Levin study (14), which covers a broad socioeconomic range, found no significant sex difference in age at toilet training, but more severe training for boys. The first of these studies is on a small sample, and the significance of differences was not determined. But if their findings are valid, then we have some corroborative evidence of differential restrictive training toward young boys and girls, according to social class.

Another point of difference between the Berkeley Growth Study and the studies with which we have been making comparisons is that the latter were made within the last 10 years, during a period when "demand schedules" and generally permissive treatment have been recommended. Havighurst and Davis (5) have conjectured that a temporal factor may account for the differences between the Harvard findings and theirs. But Maccoby and Gibbs (9) object to this explanation. In view of this controversy, it is interesting to note that our data show these socioeconomic differences to be present in 1928-32, during a period when pediatricians, nutritionists, and psychologists advised rigid schedules and restrictive training procedures. Presumably the better educated and more intelligent mothers would be more likely to follow the advice of the experts. Evidently, in this instance, changes in authoritative advice have not radically altered the pattern of socioeconomic and sex differences in maternal behavior.

E. Summary

A study of the relation of socioeconomic factors to observed maternal behavior in the Berkeley Growth Study tends to confirm findings from other studies based on interviews about childrearing practices. For the total group there was a slight tendency for the mothers of higher socioeconomic status to be more warm, understanding, and accepting, and for those of lower status to be more controlling, irritable, and punitive. These tendencies were present both during the children's first three years, and later at 9 to 14 years of age. The differences were much more evident for the mothers of boys than of girls. In the dimension of Autonomy versus Control there is some evidence for a differential socioeconomic determiner in the treatment of infant sons and daughters: that is, higher status boy babies and lower status girl babies seem to have been granted a measure of autonomy and freedom from maternal supervision.

The findings from this analysis of the Berkeley Growth Study data are in accord with other recent studies in showing that any efforts to relate maternal childrearing practices to children's personality must take into account both sex of the child and socioeconomic variables.

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COMPARISONS OF RUNNING WHEEL ACTIVITY OF NORMAL AND CHRONIC RADIATED RATS UNDER VARYING CONDITIONS OF FOOD DEPRIVATION*

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A. Problem

McDowell, Davis, and Steele (4) noted a deficit in the locomotor activity of rhesus monkeys during the first 32 days following exposure of the subjects to 400 *r* whole-body radiation with x-rays. Harlow and Moon (1), working in the lethal dose range, exposed monkeys to 100 *r* of x-radiation every 35 days until death. Their animals manifested a general decrease in locomotor activity and this decrease became greater as the roentgen dose increased. A deficit in locomotor activity in the chronic irradiated monkey has been reported by McDowell (3).

The effect of x-irradiation on the running wheel activity of the adult male rat has been studied by Jones *et al.* (2). They used single acute doses of x-rays, ranging from 200–1000 *r*. A significant deficit in activity occurred at all doses studied. Time to recovery was proportional to the x-ray dose. The subjects of this study had continued access to food throughout the period of testing.

The present study purposed to compare the volitional activity of normal and chronic irradiated male rats under varying conditions of food deprivation. The results of such an investigation would seem critical to learning studies involving chronic irradiated animals since drive is frequently defined in terms of number of hours of deprivation for the type of reward utilized.

B. Method

1. *Subjects*

Four normal and five chronic irradiated male Sprague-Dawley rats were subjects. All subjects were 137 days old when the experiment was begun.

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[1] This work was conducted at the Radiobiological Laboratory of The University of Texas and the United States Air Force, Austin, Texas, and was supported (in part) with funds provided under Contract AF 41(657)-149 and the School of Aviation Medicine.

Thirty-three days before the initiation of the experiment, each of the irradiated subjects had been exposed to 1050 *r* Co^{60} at 350 *r* per minute in the high-level Co^{60} facility at this laboratory.

2. *Apparatus*

Nine standard activity wheels were used to measure the volitional activity. The drum of each wheel was 4-1/4 inches wide and 14 inches in diameter. A Veeder-type counter attached to each wheel recorded revolutions in both directions. The living cage of each wheel measured 10 x 6 x 5 inches. An opening from the cage gave free access to the drum. Each animal had constant access to water from water bottles attached to one end of each living cage. Feeding was accomplished by placing Purina Laboratory Chow pellets on the floor of the living cage.

3. *Procedure*

The animals were maintained in the wheels and attached living cages during the entire 33 days of testing. Activity measures were recorded every day at four o'clock in the afternoon. Four food deprivation schedules were used and in the following order: (*a*) four days of total food deprivation, (*b*) 20 days of 23-hour food deprivation (with daily feeding between four and five o'clock in the afternoon), (*c*) four days of total food deprivation, and (*d*) five days of *ad libitum* feeding. All subjects had been maintained for more than 30 days on an *ad libitum* feeding schedule in individual living cages for the period just prior to the initiation of this experiment.

C. RESULTS

Figure 1 plots the mean running wheel activity per hour per group for successive days of testing under the four deprivation schedules. The mean running wheel activity per hour for each animal of each group under each of the successive conditions of testing is shown in Table 1. Statistical analysis of the data of Table 1 yielded an *F*-value for groups which was significant beyond the .05 level of confidence, an *F*-value for conditions which was significant beyond the .001 level of confidence, and an *F*-value for the groups *x* conditions interaction which was also significant beyond the .001 level of confidence. Table 2 gives the results of the analysis.

Statistical analysis of the data for each of the four-day periods of total food deprivation yielded *F*-values for days which were significant, in each case, beyond the .001 level of confidence. The mean gain from the first to the last day of the first four-day period of total food deprivation was 8.8

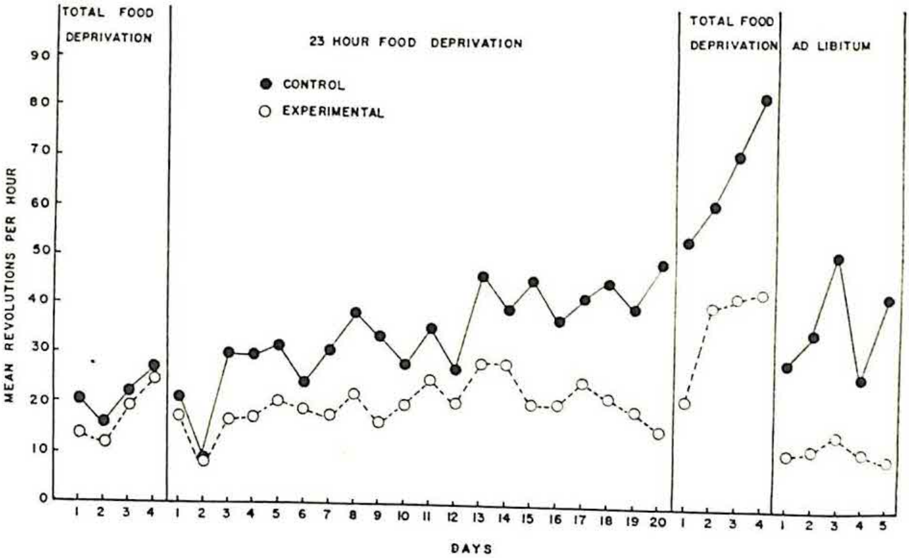

FIGURE 1
Mean Activity Wheel Revolutions Per Hour Per Day for the Animals of the Normal and Irradiated Groups Over the Four Food Deprivation Schedules

cycles per hour while that for the second four-day period of total food deprivation was 24.6 cycles per hour. The amount of gain during the second period was significantly greater beyond the .01 level of confidence than that during the first period, the significance of difference being measured by Wilcoxon's paired replicates test (5).

D. Discussion

The results of the present experiment are taken to indicate that strength of hunger drive, as measured by running wheel activity, cannot be assumed

TABLE 1
Mean Running Wheel Activity Per Hour for Each Animal of Each Group Under Each of the Four Successive Deprivation Schedules

		Schedules			
		1	2	3	4
Irradiated	1	19.6	27.7	45.9	26.2
	2	14.2	18.5	43.7	20.1
	3	26.0	29.1	50.8	4.4
	4	13.1	9.6	25.9	1.5
	5	13.5	14.5	20.0	8.6
Normal	1	26.3	46.8	88.8	46.1
	2	28.1	40.2	73.5	36.2
	3	14.1	16.7	22.0	20.5
	4	16.3	33.3	84.6	46.0

TABLE 2
TEST OF OVER-ALL SIGNIFICANCE

Source of variance	df	Sum of squares	Mean squares	F
Between-subjects	8	6,614.70		
B (Groups)	1	2,984.57	2,984.57	5.75*
Error (b)	7	3,630.13	513.59	
Within-subjects	27	7,208.70		
A (Conditions)	3	5,424.77	1,808.25	47.72**
A B	3	988.16	329.38	8.69**
Error (w)	21	795.77	37.89	
Total	35	13,823.46		

* .05 level of confidence.
** .001 level of confidence.

to be the same for the chronic irradiated as for the normal rat under similar deprivation schedules. The data suggest that the strength of hunger drive accruing to the chronic irradiated rat is less than that accruing to the normal rat when both are maintained on similar deprivation schedules.

The implications of these findings for learning studies utilizing chronic irradiated animals are immediately evident. Comparable motivation for normal and irradiated subjects cannot tacitly be assumed to be produced by similar deprivation for the incentives used in testing. Failure to take cognizance of this problem may have accounted for the wealth of negative results on the effects of radiation on learning.

The subsidiary observation of differential gain in running wheel activity between the two four-day periods of total food deprivation in the present study suggests that adaptation to the activity wheel and/or food deprivation training affects the amount of change in running wheel activity produced by a total food deprivation schedule. Activity, then, as a function of deprivation appears dependent upon learning factors as well as upon the deprivation, per se.

E. SUMMARY AND CONCLUSIONS

Four normal and five chronic irradiated male Sprague-Dawley rats, all of the same age, were compared with respect to running wheel activity under four food deprivation schedules introduced in the following order: (*a*) four days of total food deprivation, (*b*) 20 days of 23-hour food deprivation, (*c*) four days of total food deprivation, and (*d*) five days of *ad libitum* feeding.

The following results were obtained:

1. The mean running wheel activity for the animals of the normal group over the four deprivation schedules was significantly greater than that for the animals of the irradiated group.

2. Running wheel activity varied significantly with deprivation schedules.
3. The interaction between groups and deprivation schedules was significant.
4. A significantly greater gain in running wheel activity during the second than during the first four-day period of total food deprivation was noted.

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CONCEPTUAL DEVELOPMENT REFLECTED IN AGE DIFFERENCES IN ASSOCIATIONS TO NAMES AND PICTURES OF OBJECTS*

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A. PROBLEM

What happens when a person reacts to the name of an object? To a picture of the same object?

Many past free-association experiments (1, 5, 6, 7, 8) are relevant to these questions, but they have not systematically explored developmental differences nor begun to relate these to the changes in conceptualizing which occur with growth. In addition to the counting and classification of responses, there should be further speculation about the psychological processes involved.

An important aspect of the associative process is conceptual. The name or picture is identified or has meaning within the situation; it is differentiated and immediately related to other objects or aspects of experience through perceptual-conceptual processes. The purpose of the present study is to test simple suppositions about development of differences in the rate and heterogeneity of conceptualizing, using the traditional method of free association.

Studies of free association often have emphasized the regularity of response, as though the concept reflected by the verbal reply were a simple function of the stimulus word and of the frequency and contiguities of previous usage. Experimental psychologists have sometimes talked as though naming an object simply involved the *use* of a concept—as though concepts were once formed or acquired, remembered (or at least stored), ordered according to strength or value, and then employed.

In contrast is the view that each time the same object is perceived it is seen within the organization of the individual's changing world, influenced by many aspects of the ongoing cognitive process, and therefore not likely to have exactly the same meaning twice. Different as well as similar associations to the same object then become important for inferences about the process of association. And with free association considered an active process, not merely the automatic evocation of connections, the method becomes appropriate for the study of conceptualizing. It even has some advantages over

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common concept formation or attainment procedures (2, 4): the task does not require non-sense material, a great variety of responses is possible, and all of the *S*s' replies are relevant. Sometimes in the past response alternatives have been narrowly limited, and those responses which have not corresponded to the experimenter's categories have been ignored or lumped together as error.

To begin the study of the relation of associations to conceptual development certain simple variables have been selected and relationships proposed for verification. When given the name of a common object, if a child is less able to differentiate and identify its many attributes than an adult, if he is less capable of relating this part of his experience with other recent and contemporary events, then it would follow that his free association to the object would occur less readily, and would tend to be less variable from trial to trial. Similarly, it would be predicted that the associations of different children would be more homogeneous than those of adults. Also, if we presume that pictures of objects more directly provide many characteristics or details which can be categorized, and that pictures can be more readily related to memories of visual experiences, then it would be expected that associations to pictured objects would be quicker and more varied than associations to corresponding names. This difference between pictures and names would be greatest for children, for they are less adept at recognizing abstract verbal interrelationships than older persons.

Specifically, the following hypotheses were formulated:

1. Association will be faster for older subjects.
2. There will be a longer reaction time for names of objects than for their pictures.
3. The differences between associations to names and pictures will change with age.
4. The number of different responses will be greater for pictured objects than for named ones.
5. The variety of responses will be greater with increasing age.
6. The number of repeated responses to the same object on successive trials will decrease with increasing age.

B. METHOD

1. *Subjects*

Three groups of 20 *S*s each were used. Group I was selected from fourth-grade classes of a public elementary school in a suburban community. Most members of this group were 10 years old. Group II was picked from the eighth and ninth grades of the Junior High School in the same Township, and members were 13 to 15 years old. While Group III was made up of freshmen and sophomore volunteers (ages 18 to 21) from two local colleges. All groups had 10 males and 10 females, and were similar in typical *IQ* and in socio-economic status. In order that a comparison might be made with college students, only children well above average in *IQ* and school performance were selected for the younger groups. To check for similar socio-economic background the occupations of the fathers were compared.

2. *Apparatus*

To present the oral instructions, the ready signals (tones), the verbal stimuli, and the music between trials, a magnetic tape recorder was used. For visual stimuli slides were prepared, and a mechanical shutter was fastened to a conventional projector. The shutter was wired so that opening it closed the voice-key circuit, which operated both the chronoscope and a small green light at the base of a movie screen. A microphone in front of the tape recorder activated this same circuit when verbal stimuli were presented. Another microphone in front of the *S* was used in the voice-key circuit to stop the chronoscope and turn off the light when the *S* responded.

3. *Procedure*

To introduce each *S* to the experimental situation he was told by *E* that this was a word game (or word experiment for older *S*s), the apparatus was described and he was given some informal practice speaking into the microphone, loudly enough to operate the voice-key and turn off the light. Being able to extinguish a light with one's voice served as a novel experience which aroused immediate interest, especially for the younger *S*s. In later trials it tended to maintain motivation for a quick response, and it provided a constant fixation point.

To give practice in attending to the tape recorder while watching the screen, and to make sure that each member of a group was told the same thing, the task instructions were recorded on the magnetic tape. The instructions for junior high and college students were as follows:

> I shall project pictures of various objects on this screen. Interspersed with the pictures you will hear the names of various objects, which are recorded on the tape. When you see the picture or hear a name, give the first word that comes to your mind. We do not want you to give the name of the pictured object or to repeat the name you hear, but to give the first other word that comes into your mind as you look and listen. Each time the picture goes on the screen or the name is given, this light

> will go on. The sound of your voice will automatically turn the light off as you respond to the pictures and names. Five seconds before you hear a word or see a picture this tone will sound: (Tone). Please say your responses into the microphone in a clear, loud voice. Remember to say the first word that comes to your mind. If you have any questions about the nature of this experiment, I shall be happy to answer them at the end of this session.

The instructions for Group I (4th grade) had the same content as those for the older groups, but simpler language was used and there was more repetition.

All *S*s were given the same practice trials. Five seconds after the ready signal a picture of a roller skate was flashed on the screen for one second. The *S*'s response and the association time were recorded. Fifteen seconds later the tone sounded again, followed in five seconds by the spoken word 'table.' The next stimulus was the word 'house,' and the last of the series was a picture of a hammer. The practice series made it possible for *E* to determine whether the instructions had been understood, and it established an appropriate set for the task.

In the first trial that followed 10 names and 10 pictures of objects[1] were presented in random order, by the same method and with the same timing as in the practice series. For the second and third trials the same stimuli were presented but in different random orders. During the two-minute interval between trials, while *E* arranged the order of the slides, lively music was played to maintain the attention of the *S*s. Half of the *S*s at each age level had the names of one set of objects and the pictures of a second set of objects, while the other half had pictures of the first set and names of the second. Common objects were selected as stimuli so that nearly all *S*s would have no difficulty in identification. To provide a range of familiarity, less often seen objects such as a trumpet, ladder, and movie projector were included as well as everyday objects such as a clock, chair, and hat. The names of the objects in each of the two sets had about the same number of syllables.

At the end of each *S*'s trials an inquiry was conducted for evaluative purposes. He was asked whether he had been told anything about the game (or experiment) beforehand. The next question, phrased differently for younger and older *S*s, asked what he thought was the purpose of the experiment. Thirdly, he was asked how he arrived at or picked his answer. And finally he was asked whether he had had any difficulty understanding any of the spoken names or recognizing any of the pictures. For *E*'s use in deciding whether to discard the data of a wayward *S*, each *S* was rated on coöperativeness and apparent comprehension.

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[1] For the first trial for one group of *S*s the objects were given in the following order, with italics indicating pictures: *Wrench,* chair, *Toaster,* light bulb, *Lamp, Fountain Pen,* binoculars, hat, *Scale,* fishing reel, bicycle, adding machine, *Outboard Motor, Movie Projector, Clock,* refrigerator, *Ladder,* trumpet, wheel barrow, *Shoe.*

C. RESULTS AND DISCUSSION

The youngest *S*s did take significantly longer than the two older groups to associate to representations of the same objects (see group totals, Table 1), with an average difference of almost one second. The demonstration of a shorter latency for the older *S*s, the confirmation of the first hypothesis, suggests a difference in psychological processes. Why does the difference occur? One explanation might be proposed in terms of a microgenetic approach to perception (3). There may be developmental differences in the time required for the percept to achieve well-differentiated "good" form. Another explanation could be proffered in terms of the differences in the conceptual process through which the name or picture is identified. The older *S* could more readily conceive of many abstract qualities which he would immediately relate to the object and which would provide innumerable instances for association. The younger child might more often be limited to concrete classifications of function, or be more dependent upon recalling a specific experience with the object. The two explanations are not mutually exclusive. Further evidence is necessary.

TABLE 1
MEAN ASSOCIATION TIMES (SECONDS) BY AGE AND MODE OF OBJECT REPRESENTATION

	Names	Pictures	Mean
Group I Ages: 9-10 *N* = 20	2.97	2.64	2.81
Group II Ages: 13-15 *N* = 20	1.92	1.88	1.90
Group III Ages: 18-21 *N* = 20	1.86	1.85	1.85

The second hypothesis, that there would be longer reaction times to names of objects than to their pictures, was also confirmed (see Tables 1 and 2). This finding supports the explanation in terms of conceptualizing, upon which the prediction was based. Also it brings into question Karwoski's contrary generalization that associations to names are more rapid because responding to pictures requires an additional step between the perception and the verbal association (5). Karwoski did obtain longer association times to pictures. All of his *S*s, however, were college students and they responded to printed names rather than spoken ones. In the present study, for the college sub-sample alone there was no significant difference between association times to names and to pictures. The discrepancy between the results of the two investigations may be accounted for by the different methods of presenting names. Unlike printed names and pictures in which the entire stimulus pattern is given at the same time, an appreciable part of a second elapses before enough of the name is spoken for it to be identified. Because of this increment comparisons between names and pictures in this study are better stated in relative rather than absolute terms. So stated, the key finding was that in relation to their respective association times to names young children gave faster responses to pictures than did college students. There was a significant interaction between age and type of representation. As the bright child grows older there appears to be greater improvement in relating and verbally responding to names than to corresponding pictures. The third hypothesis was confirmed.

TABLE 2
ANALYSIS OF VARIANCE: INITIAL TRIAL ASSOCIATION TIMES RELATED TO MODE OF REPRESENTATION AND AGE, USING TWO SETS OF MATERIALS

Source	df	Mean square	F	Signif.
Between *S*s	59			
Age	2	114,841	24.73	$P<.001$
Representation × Sets	1	12,546	2.70	$P>.10$
Representation × Sets × Age	2	1,237	—	
Error	54	4,644		
Within *S*s	60			
Representation (Names-Pictures)	1	4,576	6.1	$P<.025$
Sets	1	1,726	2.3	$P>.10$
Representation × Age	2	3,236	4.3	$P<.05$
Sets × Age	2	3,546	4.7	$P<.05$
Error	54	756		

Contrary to expectations, however, the heterogeneity of responses was as great for names as for pictures at all age levels. Typically, for both children and college students about 10 different associations were given each word or picture by the group of *S*s that had the same stimuli, and seven of these associations were unique—used by a single *S*. The great variety of responses of the youngest *S*s indicates that the associative processes develop early; while the fact that there was no regular increase in heterogeneity with age indicates that the diversity of associations is not a simple function of the number of different experiences or of developing verbal ability. The 14-year-olds actually gave many fewer unique responses than either of the other two groups. This indication of conformity was further supported by the teen-ager's often-expressed concern during the inquiry—had he given the same answers as the other *S*s?

As to each *S*'s consistency upon repeated trials with the same stimuli, the number of college students who gave the same associations each time was surprisingly large, and so was the number of 10-year-olds who gave different associations regularly. When questioned at the end of the experiment, many of the college students said that when an item was presented again they assumed that this was a test to find out whether they could remember their first response, and that they deliberately attempted to repeat. To establish a functional relationship between repetition of associations and age, more exact control of set would be necessary.

Although no specific hypothesis was proposed relating type of association to differences in age, a classification system was devised which could be used with high reliability by the two authors. The most common types of association were those based on contiguity, function, descriptive detail, and abstract quality. For college students associations based on contiguity were the most common type, while for the youngest *S*s associations based on function occurred most frequently. This relationship held for both names and pictures, and as the 14-year-old group had more associations of the contiguity type than the younger *S*s and fewer of the functional type, a definite trend was indicated. There was also an increase in associations based on abstract quality (e.g., clock-time) with increased age. The different types of predominant associations may be attributed to different sets or interpretations of the requirements of the task, or they may indicate that the objects have acquired different meanings, that different conceptual interrelationships have been established. To prove the latter, and to determine whether differences in free association represent general differences in thought and language, would be the next step in research.

D. SUMMARY

For bright *S*s at three age levels free associations to names and pictures of common objects were obtained. Differences in the rate, heterogeneity, repetition, and type of associations were described and interpreted mainly in terms of conceptual development. Association times decreased with age. Free associations to pictures tended to be more rapid than those to spoken

names of the corresponding objects, especially for the 10-year-olds. Heterogeneity of response was as great for the youngest as for the oldest *S*s, and repetition did not decrease consistently with age. There was an increase in contiguity type response with age and a decrease in the number of functional or operational responses. As the superior child grows older there appears to be greater improvement in relating and verbally responding to names than to corresponding pictures. A beginning has been made in specifying and interpreting developmental trends in free association.

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PERCEIVING AS INNATELY DETERMINED*

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The notion that significant aspects of perceiving are learned is quite old and its controversial status is far from resolution. We may wonder why the growth of psychology as a science has made such meagre inroads in establishing a reliable body of interrelated facts and in inducing a uniform approach to theoretical and experimental issues. Perhaps philosophical issues and questions of value, as was the case in the nature-nurture controversy, exert a strong influence in this regard. Berkeley's *New Theory of Vision* (1709), for example, was doubtlessly shaped by a special attitude toward idealism and materialism. Nowadays, it is possible that the wish to stress the significance of the individual strengthens an empirical approach. The individual is not a passive or helpless recipient of external forces; rather, the world itself may image his needs and training. Given the subjectivism that prevails in some quarters, the actual status of the real world is thrown into doubt. Everyone seems to construct a world in line with his wishes and needs to a degree that a common measure of the collection of private worlds cannot be defined. A common measure, even if it were defined, simply would represent an excursion into a particular individual's private view of things. These attitudes, in so far as they exist, suggest that the *nativism vs. empiricism* controversy ultimately may be resolved along the lines of clarifying philosophical and other extra-scientific issues (1, p. 89). Rather than pursue this approach further, let us turn to a few of the vexing scientific issues which plague the question under discussion. But, first, to state our thesis: *All of the significant aspects of perceiving are unlearned.*[2] These aspects include pattern and depth perceptions, the so-called laws of organization, figure-ground relationship, solidity, the illusions, the constancies, the phi phenomenon, figural after-effects, and the perception of the world as

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[2] In stating and developing this thesis the writer's debt to Koffka (10) and Köhler (11) is obvious; see also Pratt (21).

upright. The scope of this thesis will be delimited by drawing a distinction between the origin and the modification of a perceptual achievement.

By origin we refer to the rôle of factors—whether innate, maturational, or experiential—which shape a perceptual process in a characteristic direction from the moment an organism comes in contact with the visual environment. By modification we refer to those changes which may occur in an already achieved perception through some learning factor. The bulk of experiments purporting to demonstrate the rôle of empirical factors actually deal with the question of modification, and not of origin. In this regard, for example, we think of experiments on memory color. The underlying assumption of such experimental work is that if a given perception is modified by some contemporary experiential factor then it must have been due to experience in the first place. Thus, Helmholtz wrote: "Whatever, therefore, can be overcome by factors of experience, we must consider as being itself the product of experience and training" (8, p. 13). This view is fallacious since from the fact of modification no direct inference about origin can be drawn. A parallel example exists in the field of genetics; the environmental modification of a trait does not preclude its genetic determination. For example, let us suppose that knowledge of an object influences the degree of size constancy; it would not follow thereby that size constancy is initially the outcome of learning. In view of the foregoing distinction, we shall restrict the thesis to questions of origin.

Partial evidential and inferential support for the thesis lies in the following. (*a*) Evidence suggests that there is a similarity, if not identity, in perceptual functioning in man and in lower animals (14) despite obvious differences in anatomical structures and in modes of responding to the environment. Such perceptual functioning common to man and lower animals includes form perception, depth perception, apparent movement, illusions, size constancy, and brightness constancy.[3] Apparently there must be some communality in the nervous tissue underlying perceiving or, alternatively, there must be common laws which govern the functioning of nervous tissue in relation to retinal stimulation. (*b*) In some species important aspects of perceptual functioning are apparent shortly after birth or hatching. Brightness constancy and size constancy have been demonstrated in the duckling at the respective ages of two days and seven days and discrimination among closely related geometrical figures before the fourth week (19; 20). Fantz has shown that chicks have marked form preferences before the age of four days (6). Within a few days after hatching chicks and ducklings can discriminate different heights (22). On a non-experimental level it is reported that the mound bird can fly immediately upon hatching and, on the same day, perch on a branch (2, p. 99). The swift, after hatching and growing to a fledgling stage, leaves the nest to spend practically its entire life on the wing. It gathers all food and nesting material, and may even mate, while in flight (13). It seems clear that the perceptual abilities of this bird must be complete at the time of leaving the nest. Such early perceptual achievements are not restricted to birds. Following a lead supplied by Carmichael (4, p. 69 f.), the writer was able to show depth discrimination (by using platforms of varying heights) in the guinea pig within six hours after birth. Carmichael observed an infant giraffe that could respond visually to its environment 20 minutes after birth (5, p. 324). Tinklepaugh and Hartman reported that a rhesus monkey reached for and grasped objects during delivery itself (27). The activities of the monkey reported on by Lashley and Watson (16) suggest that its perceptual apparatus is complete by the age of 21 days. These results, partly experimental and partly observational,[4] show that for these species perceptual functioning is due to innate factors. The two foregoing points strongly suggest that similar features of perception in higher phylogenetic levels (chimpanzee and man) are also outcomes of autochthonous factors.

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[3] For discussions of such evidence see References 14, 17, 25, 26, 28.

A principal difference among the species discussed above concerns the degree to which animals are able to function at birth or hatching. In some species, the nervous tissue is practically complete—at least in so far as the optical sector is concerned—at birth or hatching, whereas in higher species the optic system attains completion after birth. Consequently, in animals higher up in the phylogenetic series postnatal maturational processes can be confounded with concomitant experiential factors. Such experiential factors may merely supply the occasion for the unfolding of developmental processes and need not be of causal significance in the shaping of perceptions. Efforts to control postnatal experience by rearing animals under some restrictive condition (for example, light deprivation) may impede the rate of maturation or may actually lead to structural or functional impairment of the optic system (3; 6). By virtue of such impairment, the results of deprivation-experiments could not be decisive against the thesis that perceiving is determined by innate mechanisms. In this regard it is interesting to note the word of caution stated by Spalding in 1872, perhaps the first investigator to use animals for the purpose of testing an empirical approach to perception. With reference to his own experiments on chicks reared under some restrictive condition, he wrote: ". . . it would seem that any early interference with the established course of their lives may completely derange their mental constitution, and give rise to an order of manifestations, perhaps totally and unaccountably different from what would have appeared under normal conditions" (23, p. 7).

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[4] Observations are invaluable in providing direction for research.

Let us consider briefly the critical term *learning*. Unfortunately, this term does not have a precise definition in general psychology and, in so far as it is related to perception, its meaning is quite vague. What are the criteria for distinguishing among learning, satiation, maturation, and adaptation? Is reinforcement an essential factor in 'learning to perceive' and does such learning exhibit generalization and extinction effects? Although discussion of these questions is a topic in its own right several issues can be defined. If we are to speak of 'learning to perceive' there must be some demonstration that different training sequences lead to different perceptual outcomes. This means that there should be specifications of the percept prior to learning and after learning. Furthermore, it would be desirable to advance some theory that would deal with the processes intervening between the initial and final perceptual states. In current discussions we usually do not find a clear specification of the percept prior to learning, the post-learning situation being quite clear. However, sometimes we do find statements, explicitly or implicitly, concerning the initial state. Some of these statements include the following: a percept is "amorphous" or "blurred"; a triangle cannot be distinguished from a circle; the percept is correlated with retinal size; the animal is "form-blind"; there is no differentiation of figure from ground; there is no seen depth; the conditions under which the phi phenomenon is obtained yield punctate percepts; a human infant sees the world upside down. With regard to mediating processes, the main task of a theory of learning is to show how the initial perceptions are transformed into the perceptions we know them to be. *S-S* theory and *S-R* reinforcement theory are the two main candidates advanced for dealing with this task. Both, I believe, are inadequate as presently formulated. Both theories assume initial perceptual variability in relation to a proximal stimulus. For instance, it has been suggested that a percept, prior to the learning which is presumed to lead to size constancy, is determined by the visual angle (10, p. 87 f.). From the organism's standpoint, changing distance from the distal stimulus means perceptual variability. It has been shown elsewhere that the effect of such an assumption makes adaptive responses to stimuli impossible (18). With regard to *S-R* theory there is the additional difficulty of defining the way in which reinforcement serves to stabilize perceptions. With regard to *S-S* theory there is the problem, by no means unique to it, as to why certain aspects of the stimulus field become organized in one way rather than in some other way. For example, contour is phenomenally part of the figure and not of the ground. Yet, in a physical sense, the contour is as much a part of the ground as it is of the figure. Or, the contour (physically) is as often associated with the points interior to a figure as it is with the points exterior to the figure. On contiguity grounds alone, therefore, there is no rationale for the unique association of the contour with the enclosed points. The fact of depth perception offers a difficult obstacle for both theories since on the retina there is no identifiable stimulus correlate for depth. Appeal to eye movements or other peripheral factors also entails difficulties.

Generally, the learning approach to perception thrives on two types of discrepancies. One type pertains to the discrepancy between the proximal stimulus and the percept, as is exemplified by depth perception or by the constancies. This discrepancy necessarily forces the theorist to postulate various experiential factors—knowledge, unconscious reasoning, probabilistic weighting of cues, palpation of the stimulus object, muscular movements—providing he makes that special assumption which the gestalt psychologists have labeled as the "constancy hypothesis." Such postulated factors introduce the further question as to how they are able to effect changes in perception. The difficulties entailed by the "constancy hypothesis" can be circumvented if we accept an alternative assumption advanced by gestalt psychologists; namely, the neural medium which is the locus of a percept imposes characteristic transformations on the impulses propagated from the retinae. The outcomes of such transformations need not be isomorphic with the retinal pattern. Thus, stimulation of the retina—a two dimensional surface—can, under certain conditions, lead to a three dimensional central process. The alternative assumption, attractive as it seems to be, has not won general acceptance. One objection to it, anticipated by Helmholtz (9, p. 274), is that the investigation of perceptual processes is thereby removed from scientific scrutiny. However, Köhler's theory of satiation and the related experiments (Cf. 12), some of the experiments of Sperry and his colleagues (24), and of Lashley and his colleagues (15) would seem to weaken the force of this objection since information is obtained on the neural correlates of perceptual processes.

The second type of discrepancy is the one which supposedly obtains between the proximal stimulus and the distal stimulus. It is said, for example, that a rectangular proximal stimulus can be projected by an infinity of external configurations. Yet a specific perception can be matched to the given proximal stimulus, not an infinitude of perceptions. The theoretical import of this discrepancy for an empirical approach to perception is uncertain since it implies the "constancy hypothesis" as a hidden assumption and initial perceptual variability. Moreover, if we adopt Gibson's approach (7), one which assumes a one to one correspondence between the retinal distribution and the external visual situation, it may turn out that this discrepancy actually represents a pseudo-problem.

Certainly there are many other issues not touched upon but lack of time forbids their discussion. I should like to conclude by asserting that the most parsimonious position to take with regard to perceiving is that its major features are determined by intrinsic properties of the nervous system and that learning factors have no determinative influence. At most, learning factors must be assigned a secondary rôle; they may exert an influence but only when the perception-to-be-modified is already an existent one.

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ACCEPTANCE OF SODIUM SUCARYL BY C57 BLACK MICE*

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A. Problem

This study deals with the relative preferences for various concentrations of sodium sucaryl over water shown by C57 black mice. Sodium sucaryl in the concentrations used in this study is known to taste sweet to human *S*s and is rejected by albino rats. The results of this study may be compared with those obtained by Murray, Wells, Kohn, and Miller (1). These workers found that rats preferred water over sodium sucaryl at all concentrations tested. The data reported suggest some potential uses for mice in exploring the rôle of such factors as taste thresholds and type of training in determining the acceptance of foods or flavored solutions.

B. Method

1. *Subjects*

The *S*s were 16 male and female mice of the C57 black strain, secured from the Roscoe B. Jackson Memorial Laboratory.

2. *Procedure*

When the *S*s were approximately 25 days old, they were removed from the regular boxes in which they had been living with their litter-mates and placed in similar but individual boxes. For two days each *S* had food and water continuously available. For the next three days two identical water tubes were inserted into the box and the regular water bottle was removed. The water tubes were made of 4 ml. culture tubes flame-treated at the tip to prevent dripping. The same pairs of tubes were used for the preference tests which followed. On each preference test one tube of each pair contained tap water, while the other member contained a solution of sodium sucaryl and tap water.

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* Received in the Editorial Office on August 18, 1958.

[1] This study was done at the Hamilton Station, Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine, during the summer of 1954. M. P. Smith was a Carnegie Fellow and S. Ross is a Scientific Associate of the laboratory. We express our thanks to the Laboratory for the opportunity to carry out this study, and to Dr. A. E. Osterberg, Abbott Laboratories, Chicago, Illinois, for supplying the sodium sucaryl.

The amount of liquid consumed from each tube was measured to the nearest 0.1 ml. Each day's test began at 1 P.M. and ended at 10 A.M. During this period the tubes were continuously present while the regular water bottle was present only during the remaining three hours of the day. Purina Laboratory Chow was always present in the boxes.

Half of the *S*s was tested on a descending order of sucaryl concentrations and the other half was tested on an ascending order. The different concentrations of sucaryl used on successive days were: 0.25, 0.50, 1.00, 1.50, and 3.50 gm. sucaryl/100 ml. of water. The relative position of the tubes was systematically varied from day-to-day control possible position preferences.

C. Results and Discussion

The median consumption of water for all *S*s at the several concentrations of sodium sucaryl used is shown in Figure 1. Medians are used because of the failure to have determinant measures for all *S*s on every day. On a few occasions some *S*s consumed the entire contents of one or the other tube. Figure 1 shows clearly that preferences are related to concentrations of sucaryl. The data suggests that sodium sucaryl in concentrations around 1.00 gm./100 ml. of water may be preferred to water, while at other concentrations there either is no preference or a preference for water. A statistical test of the hypothesis that the five differences between consumption of water and sodium sucaryl could have been drawn at random from an identical population has to be rejected at the .01 level, using the Friedmann test (2).

In Figure 2 are plotted the median differences between sucaryl and water consumption at each sucaryl concentration tested. The data from the *S*s tested in a descending order of sucaryl concentrations reveal slight to large preferences for water over sucaryl at all concentrations. On the other hand, *S*s tested in an ascending order show preferences for sucaryl except at the 3.5 concentration. The curves, while approximately parallel, nevertheless, are strikingly different. The Wilcoxon *T* test of the difference between the groups at the 1.0 concentration is significant at the .05 level. It is not known why such an appreciable order effect is obtained. Such factors as chance, lingering after-tastes, direct physiological effects, and learning, among others, may be involved.

Another observation made in this study concerns the variability between *S*s. Although the *S*s are from a strain which has long been inbred, there still are considerable individual differences in taste preferences for sodium sucaryl. For instance two *S*s preferred water to sucaryl at all concentrations, and three *S*s preferred sucaryl to water at all concentrations except 3.5.

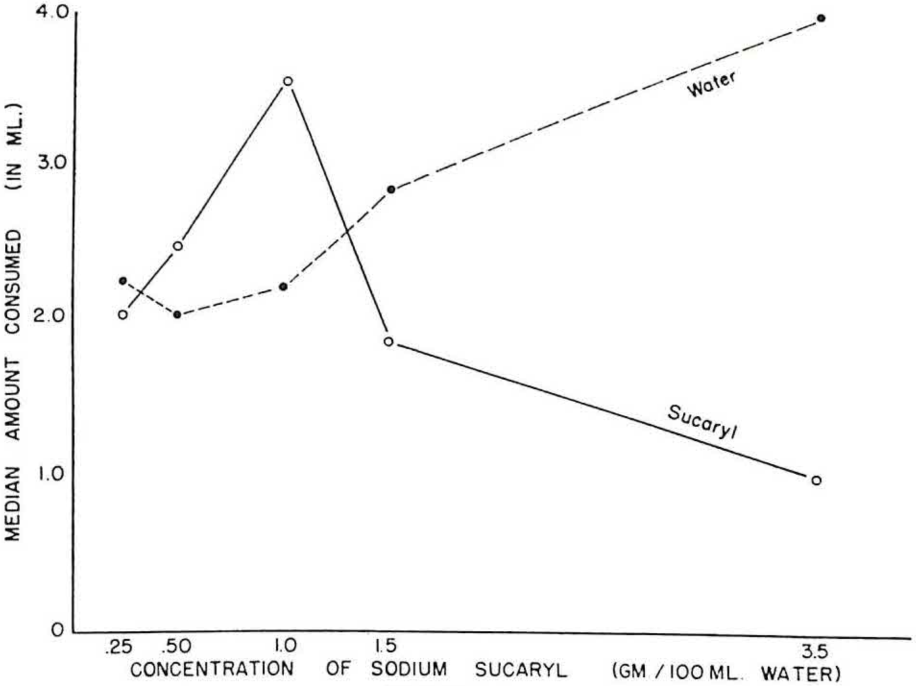

FIGURE 1

The Median Consumption of Water and Sodium Sucaryl in Various Concentrations

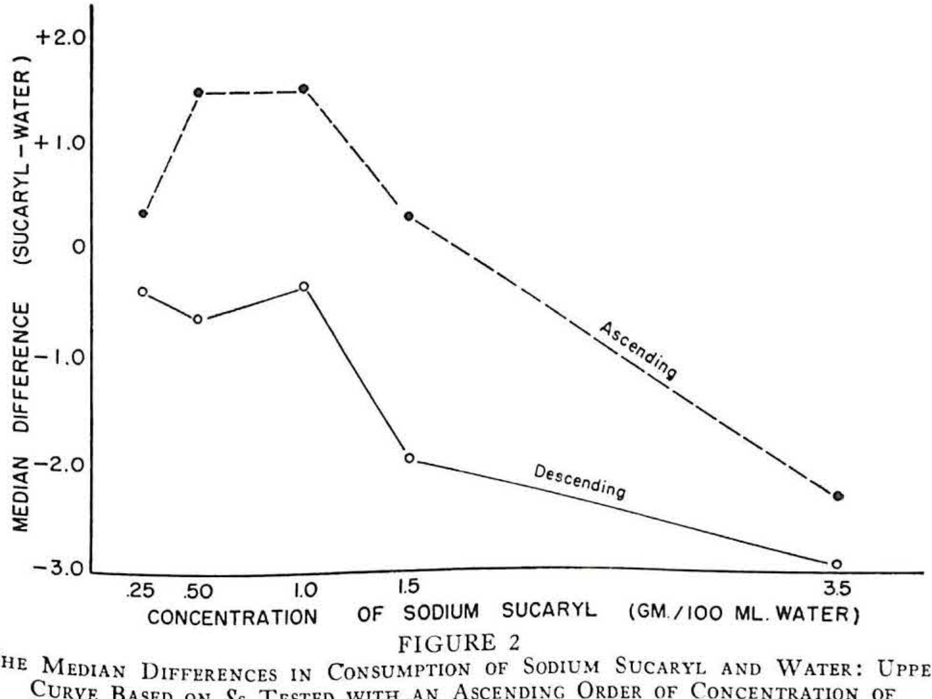

FIGURE 2

The Median Differences in Consumption of Sodium Sucaryl and Water: Upper Curve Based on *Ss* Tested with an Ascending Order of Concentration of Sodium Sucaryl; Lower Curve Based on *Ss* Tested in a Descending Order

Exactly what may account for the individual differences is unknown at the present time.

D. SUMMARY

The relative preferences of 16 C57 black mice for water and sodium sucaryl in various concentrations were measured. After five days habituation to individual living cages, mice were permitted to drink freely for 21 hours at a time from tubes, one containing water and the other a sodium sucaryl solution of one of the following concentrations: 0.25, 0.50, 1.00, 1.50, and 3.50 gm./100 ml. water. All *S*s were tested at every concentration with half of the mice tested in an ascending order and the other half in a descending order.

The data indicate an increasing acceptance of sucaryl with increases in concentrations up to 1.00 gm./100 ml. With further concentration consumption of sucaryl dropped off. An appreciable order effect was observed. *S*s tested in an ascending order showed considerable more acceptance of sucaryl than *S*s tested in a descending order. Marked preference differences between individuals were found.

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DISTRIBUTION OF TRAINING AND REVERSAL LEARNING BY CATS*

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A. PROBLEM

The effects of trial spacing on learning have been studied by varying either the number of trials given per day (1) or the interval between trials (3). The primary purpose of the present experiments was to determine the influence of these variables on cats' performance on successive reversals of a simple discrimination habit.

In addition, learning by cats tested in the Wisconsin General Test Apparatus and in a *T*-maze was compared, and the effect of prolonged experience with other types of learning problems upon retest performance on the reversal series was determined.

B. METHOD

1. *Subjects*

A total of 50 experimentally naïve cats served in the present experiments. All of the *S*s were adapted to the experimental situations by eight days' testing for lateral preferences (5), but had not been tested on any learning task. Throughout the experiment, the cats had unlimited access to water and dry dog food in their individual living cages; raw kidney, a highly preferred food, was the incentive used in testing.

2. *Apparatus*

The Wisconsin General Test Apparatus (2, Figure 1) has been described in previous publications.

The stem and both arms of the *T*-maze were 6 ft. long; the alleys were 18 in. wide, and the walls were 30 in. high. The walls of the stem of the *T* were grey; the right and left arms of the maze were black and white, respectively.

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[1] This research was supported by Grants M-835 and M-1364 from the National Institute of Mental Health, U. S. Public Health Service.

3. *Stimuli*

The objects used in the *WGTA* were a white rectangle, 1½ in. thick and 5 sq. in. in area, and a black *T*, ¾ in. thick and 11 sq. in. in area. The white object was always presented on the left, the black object always on the right, upon a grey test tray containing two food wells 12 in. apart. In both the maze and the *WGTA* there was, then, a constant association of brightness and positional cues.

4. *Procedure*

On the first training trial, neither stimulus was rewarded. *S* was required on subsequent trials to choose the initially nonpreferred stimulus and training was continued until the criterion of 11 correct out of 12 responses, with the last 8 all correct, was attained. Upon completion of this preliminary discrimination, the cat was required to select the previously incorrect stimulus. Thereafter, the two stimuli became alternately correct and incorrect as the above criterion was satisfied. The conditions of training for the several groups of cats are described below.

a. Experiment 1. Four groups of cats were trained on a series of 20 reversals in the *WGTA*. Group I (17 *S*s) learned one reversal a day with an intertrial interval (*ITI*) of 10 seconds, the minimum permitted by the apparatus. Group II (8 *S*s) also learned one reversal per day, but with an *ITI* of 70 seconds. The eight cats in Group III were given four trials per day, with 10 seconds between trials, and the five *S*s in Group IV also received four trials per day, but with an *ITI* of 70 seconds.

b. Experiment 2. Group V (5 cats) and Group VI (7 cats) were tested for 30 consecutive days in the *T*-maze. The *ITI* was 10 seconds for both groups. Group V learned one reversal per day, and Group VI learned two per day.

c. Retest. Twenty-one of the cats were retested under the same conditions which obtained for Group I during original learning. The retest was administered approximately five months after original learning, and the *S*s included both cats who had originally worked in the *T*-maze and the *WGTA*. During the interval preceding retest, *S*s were trained on approximately 130 discrimination and discrimination reversal problems.

The noncorrection method was used in all tests.

C. RESULTS

1. *Experiment 1*

The results obtained from the groups tested in the *WGTA* are summarized in Figure 1, in which mean errors to criterion are plotted against successive blocks of four reversals, with training conditions as the parameter. Groups III and IV, which received only four trials per day, are markedly inferior to Groups I and II, which learned one reversal each day, early in learning, but the curves for all groups approach one another closely in the final stages of the experiment. Analysis of variance indicates that Reversal Blocks, number of Trials, and the interaction *RB* x *T* are statistically significant sources of variation. Neither *ITI* nor any interaction involving this term is significant, so the error scores for Groups I and II and for III and IV were combined and median tests performed to evaluate the significance of the differences between the groups tested four trials and to criterion each day. These tests indicate that Groups III and IV made significantly more errors than Groups I and II on the first, second, and third blocks of reversals. Intergroup differences were not significant on the last two blocks.

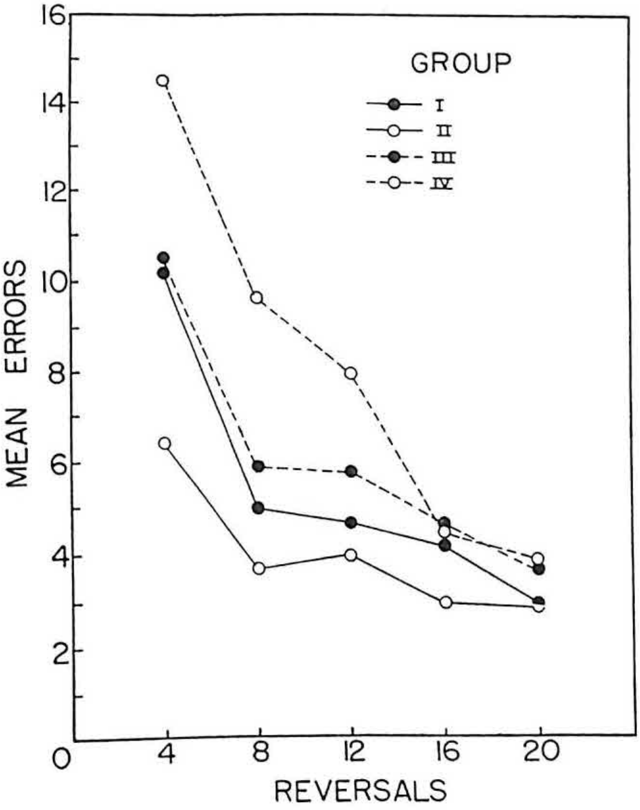

FIGURE 1
INTER-REVERSAL LEARNING AS A FUNCTION OF INTERTRIAL INTERVAL AND NUMBER OF TRIALS

Intra-reversal learning by the groups trained to criterion and the groups tested four trials each day is shown in Figure 2, which indicates the percentage of correct responses on Trials 1 through 12. The panels on the left and right present the data obtained in the first and second sets of 10 reversals, respectively; dotted lines indicate the 24 hours which intervened between successive blocks of four trials for Groups III and IV.

This figure reveals two constant differences between the performance of Groups I and II and Groups III and IV: the cats trained to criterion each day make more correct responses on Trial 1 than do the groups tested four trials a day. The superior performance of Groups I and II on Trial 1 probably reflects the fact that these animals had a potentially more reliable cue for anticipating reversals; the reward value of the stimuli was always reversed from one testing session to the next, while the same stimulus remained correct from one day to the next far more frequently than not for Groups III and IV.

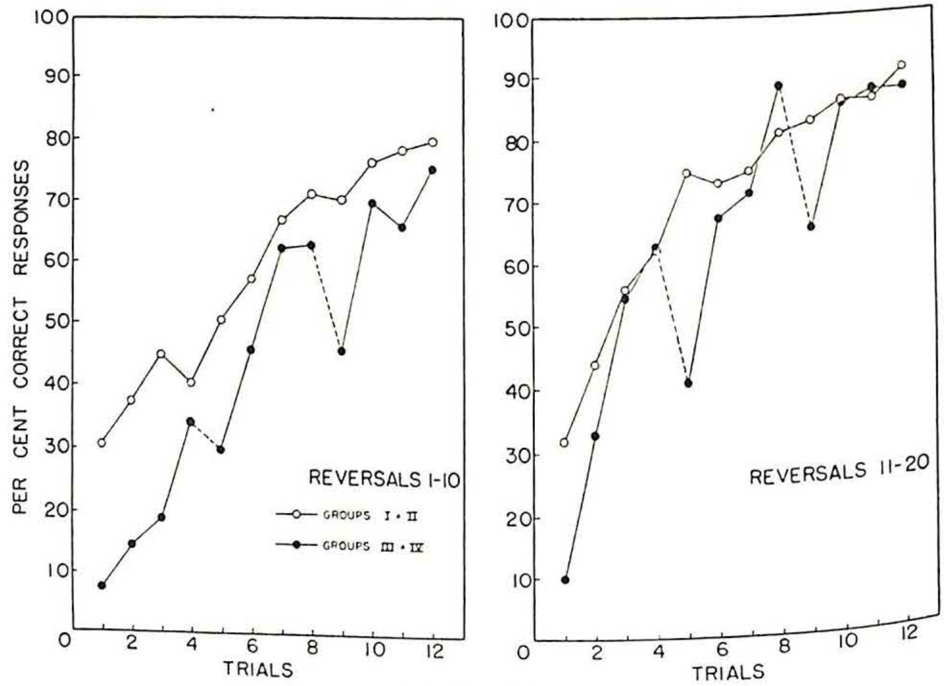

FIGURE 2
INTRA-REVERSAL LEARNING AS A FUNCTION OF NUMBER OF TRIALS PER DAY

It will also be noted that there are pronounced dips in the curves for Groups III and IV between the daily blocks of four trials each. Corresponding inversions are not present in the functions describing the performance of

Groups I and II. The regressions in performance correlated with the interruption of testing for a day suggests spontaneous recovery of the previously correct response, which must now be inhibited, between extinction sessions.

Comparison of the right and left panels of Figure 2 reveals two changes between the first and second halves of training. The generally higher level of performance by both Groups I and II and Groups III and IV is to be expected. The more striking change is the relatively greater increase in the rate of learning by Groups III and IV. During the first half of the experiment these groups were retarded relative to the groups trained to criterion, the curves for Groups I and II and Groups III and IV being approximately parallel (except on Trial 9). In the second half of the experiment, Groups III and IV are markedly inferior to Groups I and II on the first, fifth, and ninth trials, but the rate of intertrial improvement by these cats is now such that they attain about the same level of performance as Groups I and II on Trials 4, 8, and 12.

2. *Experiment 2*

The performance of the cats tested in the *T*-maze is summarized in Figures 3 and 4; these graphs represent both intra- and inter-reversal learning, since per cent correct responses are given as a function of trials, with the parameter being successive blocks of 10 reversals.

Because the *S*s in Group VI were given two reversals each day, the same stimulus was rewarded at the beginning of every testing session. That the cats could learn rather quickly that a given alternative was always correct at the beginning of a day's training is apparent from the data summarized in Figure 3, which is based on the first daily reversal for Group VI. Even the initial value of 50 per cent correct on Trial 1 is much higher than would otherwise be expected on a reversal task. A tendency to anticipate the second daily reversal is indicated by the curves for Reversals 11 to 20, and 21 to 30. On both of these blocks of reversals, performance on Trial 12 was inferior to performance on Trial 2.

Because of the unique learning curves generated by Group VI on the first reversal of each day, the most sensible comparison between Groups V and VI is that between the second daily reversal for Group VI and the single reversal for Group V. This comparison permits testing the possibilities of fatigue or warm up, since Group VI differs from V in having previously learned one reversal each day. The curves shown in Figure 4 do not suggest any significant effect associated with training the cats on two, rather than one, reversal per day, a conclusion supported by analysis of variance. Variation among reversal blocks was statistically significant; inspection of

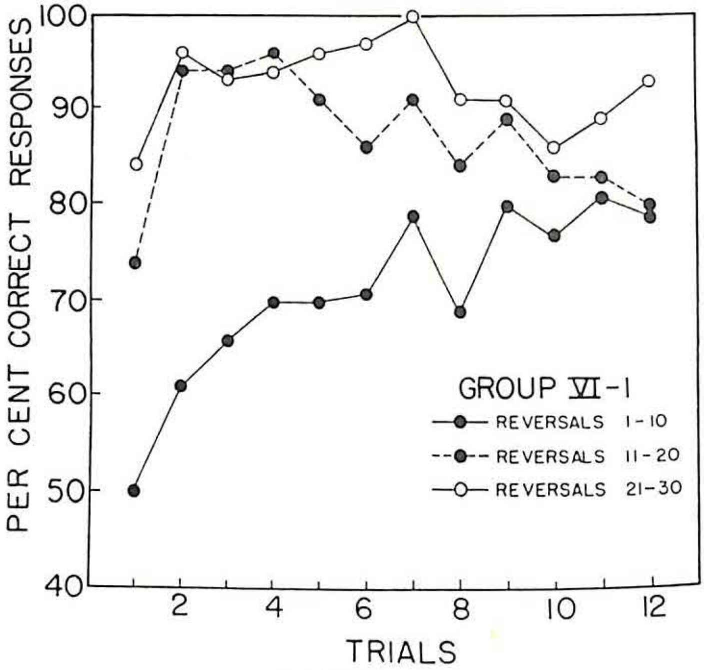

FIGURE 3
INTER- AND INTRA-REVERSAL LEARNING BY GROUP VI ON FIRST DAILY REVERSAL

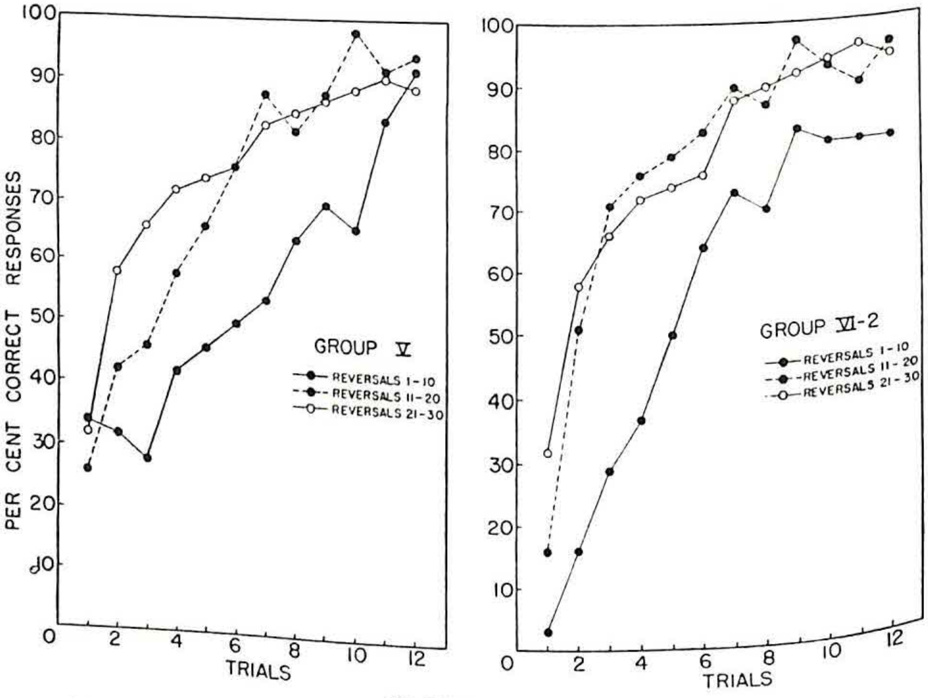

FIGURE 4
INTER- AND INTRA-REVERSAL LEARNING BY GROUP V AND GROUP VI ON SECOND DAILY REVERSAL

Figure 4 shows that the greatest improvement in learning occurred between Reversals 1 to 10 and 11 to 20.

The training given Groups I and V was quite similar except that different sensory and motor adjustments were required in the maze and *WGTA*. The performance of these groups is compared in Figure 5, in which mean error scores are plotted against successive blocks of five reversals. No comment on the virtual coincidence of the two curves after the first reversal block is required.

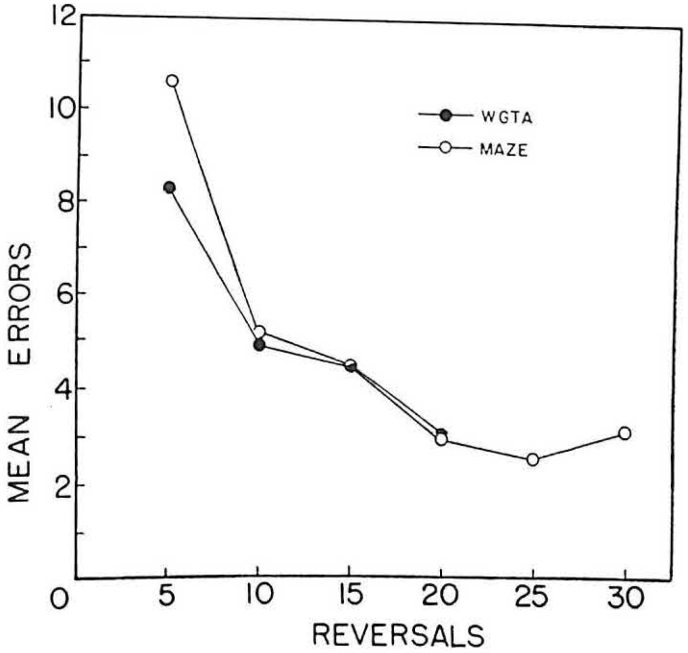

FIGURE 5
INTER-REVERSAL LEARNING BY CATS TESTED IN THE WISCONSIN GENERAL TEST APPARATUS AND *T*-MAZE

3. *Retest*

Each of the 21 cats retested on the reversal series after training on other discrimination tasks made fewer errors than in original learning. The magnitude of the improvement in performance on the retest series is indicated in Figure 6, which shows the performance of 12 cats from Group I who were tested twice under identical conditions. These cats made almost as few errors on the first four reversals of the retest as they did on the last four during original learning, and they improved continuously throughout training on the second series.

A generalized increase in rate of learning, rather than specific retention of original learning is suggested by the fact that the performance curve for four cats originally trained in the maze is nearly identical with that of the *S*s retrained under the conditions of original learning.

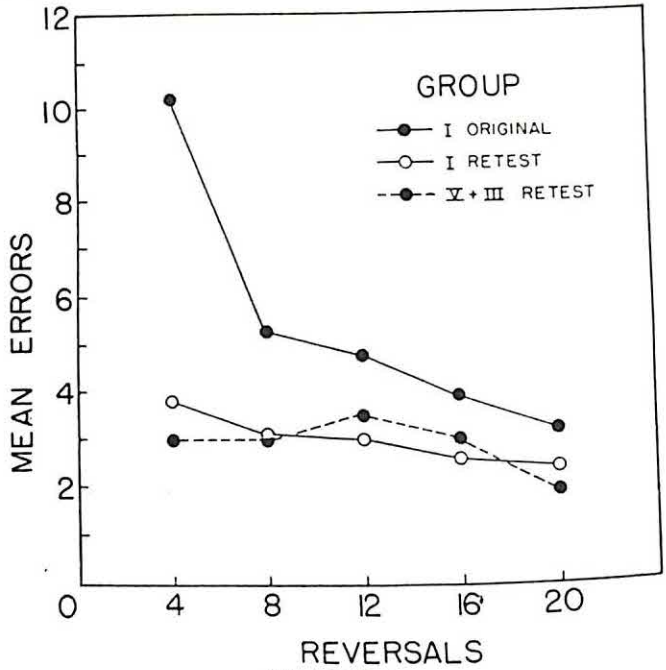

FIGURE 6
INTER-REVERSAL LEARNING ON ORIGINAL LEARNING AND RELEARNING SERIES
(*Erratum:* In legend substitute VI for III.)

D. CONCLUSIONS

Qualitatively, the results of the present experiments are in accord with the findings of somewhat comparable investigations with other species. Riopelle and Churukian (3) found no consistent relation between short *ITI*s and discrimination learning in monkeys. No significant differences between learning with *ITI*s of 10 and 70 seconds was observed in cats. Estes and Lauer (1) found that rats given four trials per day solve reversals more quickly than *S*s given only one or two trials per day. The finding that cats trained criterion each day learn more rapidly than *S*s given only four trials per day is a reasonable extrapolation from their results. Estes and Lauer did not observe any improvement in the rate of learning between days over a series of five reversals in any of their groups of rats, and did not consider it possible to determine the possibility of such improvement because of the short life span of the rat. The data presented in Figure 2 show quite clearly that cats tested four trials per day do increase the rate of learning between, as well as within, days over a series of 20 reversals. The results of the retest further suggest that the parameters of inter- and intra-reversal learning may change over a period of several months.

The present experiment suggests that there are definite limits to the beneficial effects of massing of training on reversal learning by cats. No advantage accrues from testing two reversals per day rather than one, and unpublished experiments indicate that cats make significantly more errors per reversal when tested on three reversals a day than when tested on only one.

The finding that cats learn reversals with equal facility in the maze and *WGTA* confirms the previous observation (4) of no difference in the rate with which cats solve double alternation when required to make manipulatory or locomotor responses.

E. SUMMARY

Six groups of experimentally naïve cats, totalling 50 animals in all, were tested on successive reversals of a simple discrimination problem. The following results were obtained:

1. No significant differences were obtained between groups of cats tested with intertrial intervals of 10 and 70 seconds.
2. Early in learning, groups of cats tested only four trials a day were inferior to groups trained to criterion each day; but the groups did not differ significantly in learning later reversals.
3. The performance of cats which learned one reversal per day was not significantly different from that of cats which learned two reversals a day.
4. With other conditions constant, cats learned as quickly in the Wisconsin General Test Apparatus as in a *T*-maze.
5. A large reduction in error scores was observed on a retest series of reversals administered after the cats had had extensive training on other discrimination tasks.

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TRAITS OF FATHERHOOD AS REVEALED BY THE FACTOR-ANALYSIS OF A PARENT ATTITUDE SCALE*

Child Guidance and Speech Correction Clinic, Jacksonville University, and The Rand Corporation

RALPH MASON DREGER AND ANDERS SWEETLAND[1]

A. PROBLEM

Most attempts to obtain measurement of parent-child relations, at least as perceived by those involved, have been approached from the standpoint of parents, but a need exists also for measuring the other aspect of the diadic relation. Jourard (4) recently, in endeavoring to determine the relations among "identification, parent-cathexis, and self-esteem," found it necessary to devise a questionnaire for assessing parent-cathexis, the attitudes of *S*'s toward their parents. Already, however, the instrument utilized in the investigation reported in the present paper had been developed independently but remained unpublished. Part of a "Personal Data Sheet" utilized at the University of Southern California (6) for a clinical screening device, the "Characterization of Parent," a copy of which is found in Figure 1, has been shown by one of us to parallel projective tests in measuring certain attitudes (3). In the course of investigations making use of this series of scales the question arose whether it actually tapped underlying dimensions of parent-directed attitudes. It is this question which the factor analysis described here is intended to answer. This inquiry is a portion of a larger investigation to determine what dimensions of parenthood there are as far as can be determined by scaling methods.

B. PROCEDURE

Three hundred and fifty students in the elementary psychology classes of a Southern university were administered the "Characterization of Parent" under as nearly standard conditions as possible. Only the form of the scales calling for attitudes toward the father was employed, although an alternate form for characterization of the mother is found on the original "Personal Data Sheet."[2] Scales were scored from 1.0 to 8.0, from unfavorable to fa-

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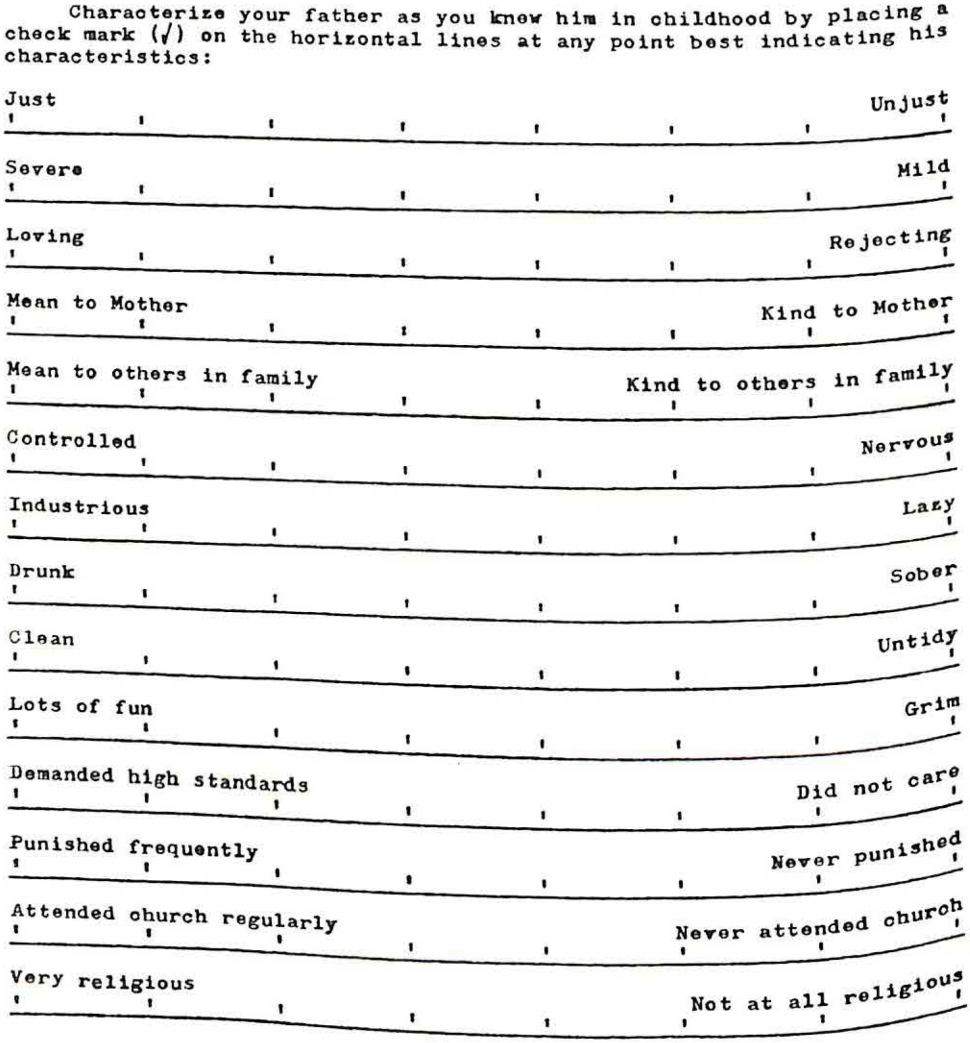
[1] Acknowledgment is made to the staff of the Department of Psychology, Florida State University, who kindly assisted in gathering the data for this study.

[2] Research on attitudes toward mother is currently under way in the university with which one of us is associated now.

vorable as defined by our culture. Only on Item 12 is there much question as to which end of the scale is culturally more acceptable: in this case the end of the scale marked "Never punished" was scored as favorable. Distributions obtained are not entirely normal in several cases, yet for the kind of items which are found here there is a surprising spread and normality of distribution in most cases. Results, however, must be tempered by the recognition that some of the assumptions of Pearson-type correlations are not fulfilled in several instances.



Characterize your father as you knew him in childhood by placing a check mark (√) on the horizontal lines at any point best indicating his characteristics:

Just	Unjust
Severe	Mild
Loving	Rejecting
Mean to Mother	Kind to Mother
Mean to others in family	Kind to others in family
Controlled	Nervous
Industrious	Lazy
Drunk	Sober
Clean	Untidy
Lots of fun	Grim
Demanded high standards	Did not care
Punished frequently	Never punished
Attended church regularly	Never attended church
Very religious	Not at all religious

FIGURE 1
RATING SCALES CHARACTERIZING PARENT AS RECALLED FROM CHILDHOOD

Because *IBM* equipment was not available, the authors devised a method of obtaining cell counts for tetrachoric correlations by means of weighing rather than counting of cards, enabling the 196 correlations to be secured in a comparatively short time with the Cheshire-Saffir-Thurstone nomograms. The method of weights is described in another article (8).

TABLE 1
INTERCORRELATIONS OF "CHARACTERIZATIONS OF PARENT" SCALE ITEMS

							Items						
1	2	3	4	5	6	7	8	9	10	11	12	13	14
1	.42	.69	.54	.48	.47	.58	.32	.51	.54	.36	.24	.59	.21
2		.43	.46	.38	.35	.28	.28	.20	.34	.26	.41	.01	—.07
3			.57	.57	.42	.54	.37	.52	.62	.41	.25	.29	.39
4				.79	.34	.55	.51	.52	.57	.36	.16	.38	.32
5					.23	.52	.64	.45	.50	.42	.07	.37	.28
6						.32	.15	.31	.40	.31	.15	.17	.17
7							.47	.69	.38	.63	.09	.28	.27
8								.47	.28	.27	.31	.46	.35
9									.49	.59	.12	.31	.30
10										.34	.01	.35	.37
11											.37	.25	.32
12												—.10	—.05
13													.86
14													

A centroid analysis made of the correlation matrix (Table 1) resulted in seven factors in the unrotated factor matrix (Table 2). The frequency distribution of residuals in the eighth residual matrix is approximately normal about a mean of zero with a standard deviation of only 0.0335, indicating that virtually all of the non-chance variance had been removed. Twenty-one orthogonal rotations of the axes were performed by means of a graphic rotation method employing transparent graph paper marked off for direct reading on the new axes. Use of the graphic method brings about some discrepancies between communalities in the rotated and unrotated factor matrices, but in no case of more than a magnitude of 0.08, which must be recognized as an accumulation of small differences squared. The major criterion for rotation was simple structure as defined by Thurstone (9) though positive manifold was kept in mind inasmuch as only chance negative correlations were found in the original correlation matrix. It is obvious, however, from an examination of the rotated matrix (Table 3) that one factor contains non-chance negative factor loadings. In two cases, if 0.30 is taken as an upper limit of chance loadings, simple structure could not be achieved as well as might be hoped. Psychological meaning was not taken into account in rotation, partly because there has been no previous experience in factoring the kind of scale utilized in this investigation.

TABLE 2
UNROTATED FACTOR MATRIX OF CHARACTERIZATION OF PARENT SCALE

Item	I	II	III	IV	V	VI	VII	h²
1	759	182	102	—291	—274	252	—280	921
2	481	293	—406	—200	034	127	095	548
3	772	138	—096	—207	—221	—212	—044	763
4	784	—167	—290	102	—182	088	129	795
5	742	—246	—347	286	—111	146	073	852
6	487	269	090	—166	—062	110	203	402
7	720	193	204	395	—114	091	029	775
8	631	—205	—189	195	283	172	—239	681
9	704	120	184	314	—093	—133	—178	700
10	658	—087	—078	—116	—270	—243	—057	595
11	631	271	206	223	223	—226	175	695
12	279	366	—236	—066	490	—046	—150	537
13	581	—563	455	—279	160	283	—136	1064
14	524	—536	381	—194	230	—229	084	857
	5739	1212	965	763	717	466	324	10185
	56.4%	11.9%	9.5%	7.5%	7.0%	4.6%	3.2%	(100%)

(Decimal points are omitted for convenience.)

TABLE 3
ROTATED FACTOR MATRIX OF CHARACTERIZATION OF PARENT SCALE

Item	I	II	III	IV	V	VI	VII	h²
1	741	038	155	502	—032	018	108	840
2	382	322	—052	090	140	031	476	508
3	659	—021	371	074	013	021	368	714
4	264	—093	306	372	022	142	640	741
5	085	—047	367	482	038	171	653	834
6	487	007	—075	074	052	252	243	374
7	352	018	388	352	—095	600	175	798
8	052	133	388	502	386	128	257	654
9	383	020	563	253	—037	392	078	689
10	440	—162	424	093	—037	—060	335	525
11	362	039	371	—055	225	598	150	704
12	191	517	106	—061	445	122	067	536
13	288	—578	118	567	483	000	—082	992
14	108	—411	631	—037	499	024	042	831
	2172	933	1757	1420	917	1021	1519	9740
	22.3%	9.6%	18.0%	14.6%	9.4%	10.5%	15.6%	(100%)

(Decimal points are omitted for convenience.)

C. RESULTS

Reference should be made to Figure 2 for factors identified by their loadings above 0.30.

Factor I is called tentatively *Ideal American Fatherhood*. Noteworthy is the paralleling of justice and love as the leading characteristics of this factor. This union is one which, in spite of what appear to be logical contradictions between the two ideals, is widely held in our culture. Here as in the other ideal fatherhood factor (III) one possessed of this attitude-trait is held to tend toward being "lots of fun." Additionally, the tendencies toward compulsiveness found in our society are represented here.

I. Ideal American Fatherhood

(1)	Just	.74
(3)	Loving	.66
(6)	Controlled	.49
(10)	Lots of fun	.44
(9)	Clean	.38
(2)	Mild	.38
(11)	Demanded high standards	.36
(7)	Industrious	.35

II. Secular Non-Punitiveness

(12)	Never punished	.52
(2)	Mild	.32
(14)	Not at all religious	-.41
(13)	Never attended church	-.58

III. Ideal Religious (non-ecclesiastical) Fatherhood

(14)	Very religious	.63
(9)	Clean	.56
(10)	Lots of fun	.42
(7)	Industrious	.39
(8)	Sober	.39
(11)	Demanded high standards	.37
(3)	Loving	.37
(5)	Kind to others in family	.37
(4)	Kind to Mother	.31

IV. Ecclesiastical Righteousness

(13)	Attended church regularly	.57
(1)	Just	.50
(8)	Sober	.50
(5)	Kind to others in family	.48
(4)	Kind to Mother	.37
(7)	Industrious	.35

V. Church-Going Religiosity

(14)	Very religious	.50
(13)	Attended church regularly	.48
(12)	Never punished	.44
(8)	Sober	.39

VI. Puritanism

(7)	Industrious	.60
(11)	Demanded high standards	.60
(9)	Clean	.39

VII. Lovingkindness

(5)	Kind to others in family	.65
(4)	Kind to Mother	.64
(2)	Mild	.48
(3)	Loving	.37
(10)	Lots of fun	.34

FIGURE 2

FACTORS OF FATHERHOOD AS DERIVED FROM RATING SCALES "CHARACTERIZATION OF PARENT"

Factor II is named *Secular Non-Punitiveness.* This is the only bipolar factor of the seven; it is identified not only by lack of severity but also by lack of religiosity.

Factor III, like its counterpart, Factor I, has substantial loadings on more items than we should expect if simple structure were achieved. However, no matter how many trial rotations were made, many beyond the 21 actually employed, these factors could not seem to release variance to other factors. Both factors make psychological sense, however, with Factor III, entitled *Ideal Religious (nonecclesiastical) Fatherhood,* fairly clearly distinguishing the father characteristics of a capitalist, religious society. It is evident that despite the high correlation between church attendance and religiosity a father may be regarded as religious without being ecclesiastical.

Factor IV, *Ecclesiastical Righteousness,* is, as suggested by loadings on similar ethical items, allied to the factor just mentioned. Yet on the previous factor no substantial loading is found on the church attendance item and on this factor none is found on the strictly religious item. In turn, it seems that a man may have a trait, at least as observed by his children, of church-goingness, as distinct from being religious.

Factor V, *Church-Going Religiosity,* puts together in one trait religiosity and church loyalty, a factor we should expect to characterize some fathers in our culture. In addition, there can apparently be a religious non-punitiveness, just as there is a secular form.

Puritanism, Factor VI, appears to represent a real trait in our culture, though it is only partially represented here most likely. The temptation of a clinician is to employ a term denoting obsession-compulsion as a name for this factor; but to designate as psychopathological anything having compulsive features is revealing only of the clinician's obsession not of the realities of the culture.

Factor VII seems to be most appropriately named *Lovingkindness.* As in the other cases where the two kindness elements are found, they are not separated here. Although the two religious items are separable, the kindness elements, even with a lower actual correlation in the original matrix, cannot be distinguished from each other as easily.

D. DISCUSSION

It is somewhat disappointing from a mathematical standpoint, but encouraging from a psychological one, to find seven factors represented in a test of only 14 items. The encouraging aspect is that the devisors of the test were astute enough psychologists to be able to include discriminating items. But as is often the case, the underlying dimensions discerned by the analysis are not those which even astute *a priori* judgment could have predicted very well. The most outstanding example of a surprising fact revealed by analysis of a pattern of correlations is seen in the separation in the case of two factors of Items 13 and 14 which in the original table are correlated 0.86. Nevertheless, the results of the overall analysis are not entirely unpredicted either in psychological theory or commonsense opinion.

There are several aspects of the analysis which should be brought to our attention which may help in interpreting the factors found here. First, it must be recalled that there are several methodological inadequacies which may vitiate to some extent the conclusions of this study. Secondly, the rankings on which the analysis is based represent something a bit unusual in such investigations; they are not self-characterizations as in ordinary temperament testing, nor are they the usual rankings of a contemporary, but instead judgments of children on their fathers' characteristics as they remember them from childhood. Thus, there is a two-fold distillation beyond self-ratings or other-ratings, i.e., childhood perceptions with all their inaccuracies and also distortions of memory; an additional complication is possible if the father is still living and has changed to any marked degree in the interim. Actually, therefore, the ratings which were made to represent characteristics of fathers may represent primarily attitudes of raters more than descriptions of fathers, attitudes molded by the culture to a great extent as well as by the experience of the raters. Nevertheless, after consideration of the features making for distortion, in the factors disclosed there remains a psychological reasonableness and enough discrimination of traits to indicate that real persons have been pictured, not merely individually and culturally distorted caricatures.

What looks like the most significant individual finding of this investigation is the separation of two ideal fatherhood factors, a secular American ideal and a religious one. Leaning to some degree more toward Ralph Waldo Emerson than Robert Ingersoll, the individual characterized strongly by the former trait seems to be an American gentleman of high principles, not evidently anti-religious but certainly not religious. On the other hand, one possessed strongly of the second trait seems not to be so marked by the firmness of principle characterizing the first, but he is very religious.

A second significant finding is the distinction between being religious and being a church-goer. A great many people believe that carrying out the ecclesiastical forms of religion is not necessarily the same as having the spirit of religion. But the fact that two different factors emerge from the analysis is a striking confirmation of this recognition.

When we return to the question which generated the study, whether the instrument used taps underlying dimensions of parent-directed attitudes, or more generally what dimensions are there to fatherhood in our culture, a conservative answer can be given, that there appear to be at least four, possibly five, fairly clearly recognizable dimensions revealed in the American fatherhood pattern, and that the instrument used does tap fundamental dimensions.

E. SUMMARY

A parent-attitude scale was administered to 350 students and analyzed by the centroid method. Orthogonal rotation of axes resulted in four important factors: I. Ideal American Fatherhood; III. Ideal Religious (non-ecclesiastical) Fatherhood; IV. Ecclesiastical Righteousness; and VII. Lovingkindness. Three other factors also appeared: II. Secular Non-Punitiveness; V. Church-Going Religiosity; and VI. Puritanism.

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THE DEVELOPMENT OF SOCIAL FACILITATION OF EATING IN PUPPIES*

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A. INTRODUCTION

A number of experiments have shown that animals eat more in group than in individual feeding. This has been demonstrated in fish (13), birds (2, 3, 12), rats (5), monkeys (6), and dogs (7, 8, 9, 10, 11). Reviews of the literature pertinent to this problem have been made by Crawford (4) and by Smith and Ross (12).

The question of whether social facilitation of eating is a natural response or is dependent on learning has been neglected. Harlow suggests that this form of social facilitation is independent of experience in rats.

A study at the University of Georgia indicated that learning was important for social facilitation in dogs. In this experiment three puppies were reared in isolation after weaning and were fed separately for 90 days, while a second group of three puppies was reared and fed together for the first 90 days of life. After this interval, both groups were tested in group and individual eating for 40 days to determine the effect of social facilitation on food intake. Those animals which were reared and fed separately did not show social facilitation at once, but did show it after two weeks experience. Those which were reared and fed together exhibited social facilitation at the beginning of the tests. It was concluded that secondary reinforcement of eating develops in the social feeding situation in dogs, and that it is specific to eating and not generalized from other forms of social activity.

In the above study social facilitation was not measured until after four months of age. It is of interest to determine how early this form of facilitation may develop in young animals.

B. PROBLEM

The problem in the present experiment was to observe the development of social facilitation in two groups of puppies as soon as they were shifted from sucking to eating.

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[1] This research was supported by the Graduate School of the University of Georgia.

C. METHOD AND PROCEDURE

A litter of 10 predominately hound puppies were used in the experiment. They were born in the psychology laboratory of the University of Georgia and remained together for the 21-days nursing period. At this time, they were separated into two groups of five animals each and placed on schedules of one feeding per day for 40 days. The food consisted of wet mash of Purina Dog Chow. On some days the animals of each group were fed separately (*S* single) and on other days were fed as a group (*T* together). The feeding schedule of Group 1 was as follows: TSSTTSSSTTSSSSS-TTTSTTTTTTTTSSTTTTSSTTSSTT. The schedule of Group 2 was just the opposite program, that is, while the members of Group 1 were fed together, those of Group 2 were fed separately and visa versa. The amount of food consumed under each condition was determined by weighing each puppy before and after eating. The animals were given plenty of time to finish in every case, and were not removed from the feeding situation until they had had an opportunity to return to the pan if they desired. Every attempt was made to rule out competition for food. A large pan was used in which each dog could eat without difficulty, and each time more food was placed in the pan than the animals could eat.

D. RESULTS

Curves have been constructed of the food intake of each animal under individual and social feeding. On examination of the curves it was quite evident that the puppies were eating more in social feeding after a number of tests. After these curves separated, the animals never did eat as much in individual as in social feeding. The curves did not separate at exactly the same time in each case, as one would expect, and in only one dog did they separate immediately. The number of days feeding under each condition before the animals began definitely to eat more in the social situation is shown in Table 1. The table also shows the number of times each animal had eaten together and separately before the appearance of social facilitation.

The average number of days before there was a definite difference in favor of social eating was 16 for Group 1, and 9.2 for Group 2. The number of days these animals were fed in social or individual feeding before this occurred can be obtained from Table 1.

Figures 1 and 2 show the curves of the average food intake for each day of both groups under *S* and *T* which have been smoothed out by inspection. This gives an over-all picture of the behavior.

The results of this study seem to substantiate the previous one that a

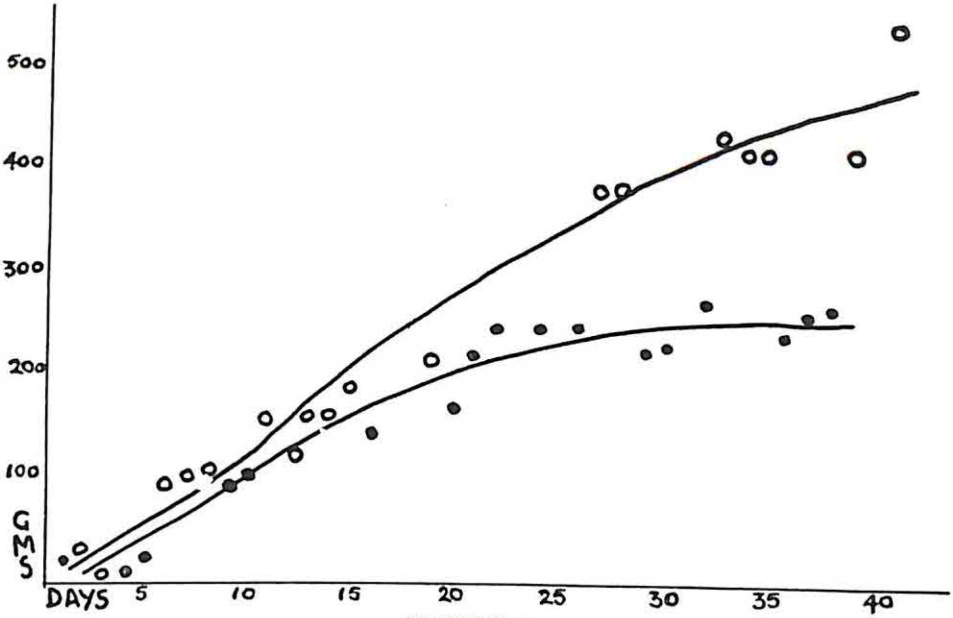

FIGURE 1
THIS FIGURE SHOWS THE AVERAGE FOOD INTAKE OF SINGLE FEEDING ●, AND SOCIAL FEEDING 0 OF GROUP 1 FOR THE 40-DAY TESTS

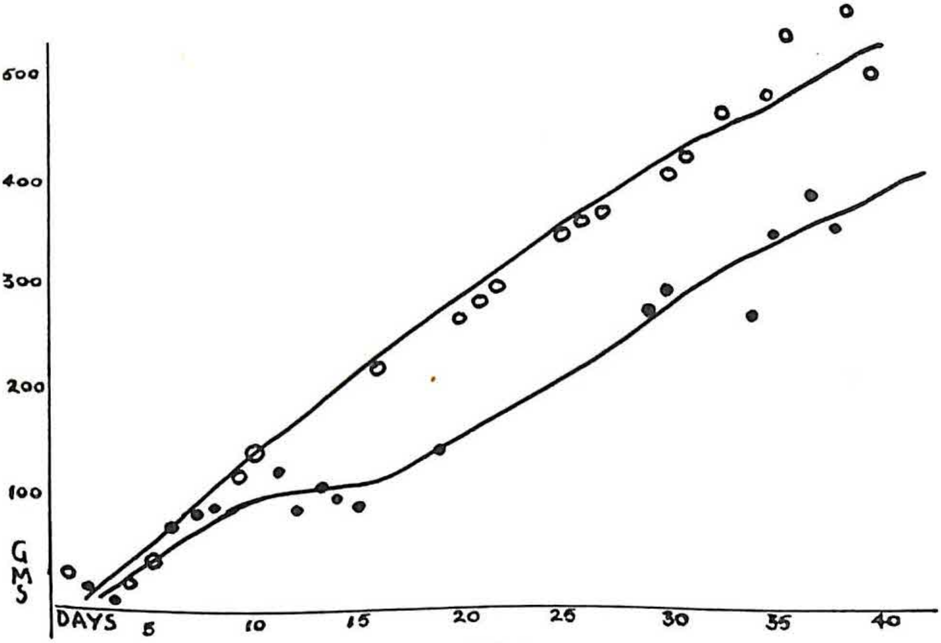

FIGURE 2
THIS FIGURE SHOWS THE AVERAGE FOOD INTAKE OF SINGLE FEEDING ●, AND SOCIAL FEEDING 0 OF GROUP 2 FOR THE 40-DAY TESTS

period of eating together is required for the development of social facilitation in puppies. Apparently some of the factors involved in group eating act as reinforcing stimuli and increase eating behavior together. It is quite evident that social facilitation develops early in young puppies.

TABLE 1
NUMBER OF DAYS INDIVIDUAL AND SOCIAL FEEDING BEFORE EACH DOG ATE MORE TOGETHER

Dog	Days for curves to separate	*S.* feeding	*T.* feeding
	Group 1		
173	19	8	11
174	12	5	7
175	26	15	11
176	3	1	2
177	22	11	11
	Group 2		
168	9	5	4
169	7	4	3
170	11	6	5
171	8	5	3
172	12	7	5

E. SUMMARY

Two groups of five puppies each were studied for the effect of social facilitation in eating as soon as they were weaned. On some days the members of each group were fed together, while on other days they were fed singly for a total of 40 days. The results indicated that social facilitation appeared after a number of social feedings in each animal except one. In one puppy it appeared on the first social feeding. Once social facilitation appeared the animals always ate more in the social situation. This study would seem to corroborate the previous one that secondary reinforcement of eating develops as the animals eat in groups and is not generalized from other forms of social activity.

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PERIPHERAL CUE LEARNING SET IN RHESUS MONKEYS*

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A. Problem

Murphy and Miller (2) have studied the effect of spatial contiguity of cue and reward in the object-quality learning of rhesus monkeys. Their animals completely failed to acquire the necessary discriminations in object-quality learning when the stimulus object was displaced six inches vertically from the site of food reward. Also, the performance of animals previously given extensive learning set training under standardized conditions decreased significantly to chance level when the spatial factor was introduced. Their animals had not had prior training on the utilization of peripheral cues to procure food rewards.

The purpose of the present study was to determine the effects of such prior training on the acquisition of peripheral cue learning sets by normal and chronic whole-body irradiated monkeys that had, also, experienced extensive learning set training under standardized conditions.

B. Methods

1. *Subjects*

Four control and nine chronic whole-body irradiated rhesus monkeys with nearly identical training histories were used as *S*s in the experiment. The nine experimental *S*s had been exposed to a mixed source of gamma and neutron radiation approximately three years before the present study was initiated.

Five of the experimental *S*s had received a total radiation dose of 154 rep and the remaining four experimental *S*s had received a total radiation dose of 308 rep.

All of the *S*s had previously experienced extensive learning set training under standardized conditions. Also, all of the *S*s had been given training on the transfer of a single learned discrimination along a peripheral cue gradient (1).

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2. *Apparatus*

All testing was conducted in a modified version of the Wisconsin General Test Apparatus, the holding cage of which measured 36 x 30 x 24 inches.

A modified two-string patterned string board, as shown in Figure 1, was used for perpiheral cue testing. This string board was placed directly over the stimulus tray used in standardized testing. The strings, pieces of plumber's chain, were attached in the front of the board to seated eye-bolts and extended to behind a three-sided chimney in the rear center of the board. Small nails attached to the free ends of each length of plumber's chain were used to impale the food reward, a raisin, on the end of the appropriate chain. The three-sided chimney prevented the subject from response on the basis of seeing which chain held the raisin. The cue positions for the peripheral cue learning set training were measured along the back of the board. The distance from the inner edge of each cue position to the edge of the three-sided chimney was 3.375 inches.

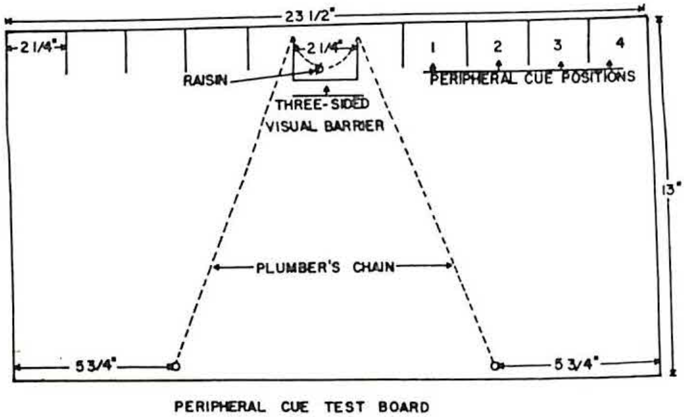

FIGURE 1
THE MODIFIED TWO-STRING PATTERNED STRING BOARD USED FOR THE PERIPHERAL CUE TESTING

The stimulus objects consisted of 234 pairs of objects, including dime-store objects and wooden objects made at the laboratory.

3. *Procedure*

Each *S* was tested on six four-trial problems per day for 39 days. The *S*s were tested at the same time each day for five consecutive days each week.

During the testing the food reward was impaled on the end of a length of plumber's chain and hidden behind the three-sided chimney in the rear center of the modified string board. Direct manual response to the length of chain on the side of the board holding the positive object was rewarded rather than direct manual response to the object itself. The non-correction method was used.

C. RESULTS

Statistical analysis of the data, as shown in Table 1, yielded (*a*) no significant difference between groups, (*b*) no significant difference between periods, and (*c*) a difference between trials within periods which was significant beyond the 0.1 per cent confidence level. Figure 2 shows the per cent errors on Trials 1, 2, 3, and 4 for each of the three successive 13-day training periods.

TABLE 1
ANALYSIS OF VARIANCE OF ERROR SCORES

Source	df	Mean squares	F	P
Between subjects	12			
Groups (A)	2	43.14	.81	NS
Error	10	53.07		
Periods (B)	2	1.12	.06	NS
A x B	4	20.39	1.05	NS
Error	20	19.45		
Trials (C)	3	131.23	32.64	.001
A x C	6	4.87	1.21	NS
Error	30	4.02		
B x C	6	8.51	.80	NS
A x B x C	12	68.52	6.43	.001
Error	60	10.66		

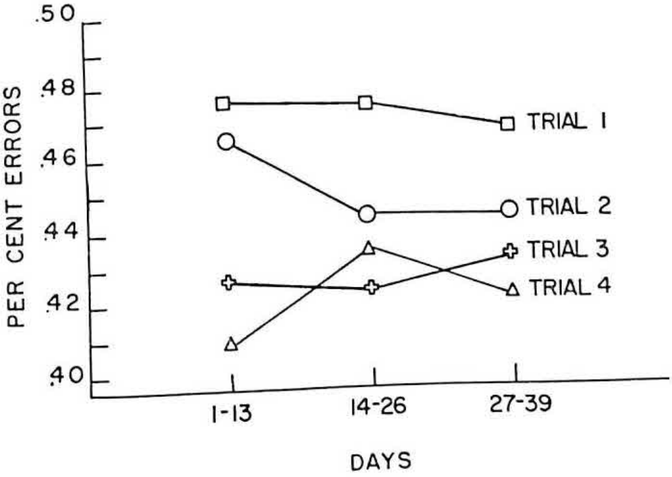

FIGURE 2
PER CENT ERRORS ON TRIALS 1, 2, 3, AND 4 FOR EACH OF THREE SUCCESSIVE 13-DAY TRAINING PERIODS

D. Discussion

In the present study, as in the work reported by Murphy and Miller (2), rhesus monkeys manifested no significant improvement in performance between periods of practice on peripheral cue learning set training. The monkeys of the present study did, however, show significant intra-problem learning. Chronic whole-body irradiated animals were not differentiated from normal animals with respect to these results.

The results are interpreted as demonstrating that both normal and chronic whole-body irradiated monkeys, with previous learning set training under standardized conditions and prior experience in the utilization of peripheral cues to procure food rewards, manifest significant intra-problem learning while failing to show inter-problem learning on peripheral cue learning set problems.

E. Summary

Four control and nine chronic whole-body irradiated rhesus monkeys, with previous experience on standardized learning set problems and in the utilization of peripheral cues to procure food rewards, were tested on six four-trial peripheral cue learning set problems per day for 39 days. The results of the experiment were that:

1. The irradiated *S*s did not differ significantly from the control *S*s with respect to performance on this training.
2. There was no significant inter-problem improvement in performance over the periods of testing.
3. There was statistically significant intra-problem improvement in performance which was consistent over the periods of testing.

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VISUAL ACUITY PERFORMANCE OF NORMAL AND CHRONIC IRRADIATED MONKEYS*

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A. Problem

A suggestion of deficit in visual acuity during the first year following radiation exposure has been reported by Davis, Elam, and McDowell (1) for monkeys receiving a total whole-body dosage of 616 rep. No evidence of such a deficit was manifest during the same period in monkeys exposed to a total whole-body radiation dosage of 308 rep, or less.

The present study proposed to investigate the visual acuity performance of the same group of monkeys approximately three years after radiation exposure. It sought to determine the persistence of the deficit noted earlier in the high dose animals and to check the possibility of deficit in the lower dose animals as a latent effect of radiation exposure.

B. Methods

1. *Subjects*

Six control and 23 chronic whole-body irradiated rhesus monkeys with nearly identical training histories were used as *S*s in the experiment. The 23 experimental *S*s had been exposed to a mixed source of gamma and neutron radiation approximately three years before the present study was initiated.

Four of the experimental *S*s (subsequently designated as the high-dose or *HD*-Group) had received a total radiation dose of 616 rep and nine (subsequently designated as the intermediate-dose or *ID*-Group) had received a total radiation dose of 308 rep. Two of the remaining 10 experimental *S*s (subsequently designated as the low-dose or *LD*-Group) had received a total radiation dose of 77 rep and the other eight had received a total radiation dose of 154 rep.

2. *Apparatus*

All testing was conducted in a modified version of the Wisconsin General Test Apparatus with the carrying cage serving as the holding cage during testing. The spatial separation between the two foodwells on the stimulus tray was 12 inches.

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3. *Procedure*

Eight separate problems were given in order of increasing difficulty, each problem requiring the subject to choose between circles and circles with breaks in order to procure a food reward. The circles with breaks are shown in Figure 1. In the first and easiest problem a circle was paired against a circle with a 90° break. The remaining seven problems, in order of presentation, involved circles with breaks of 45°, 23°, 14°, 7°, 4°, 2°, and 1°. Each stimulus card was glued to a 3-3/4 x 4-3/4 inch piece of masonite and was presented in the horizontal plane on discrimination trials. When any *S* achieved a criterion of 21 correct responses (24 trials a day) for two successive days on a given problem, he progressed to the next most difficult problem. The noncorrection method was used.

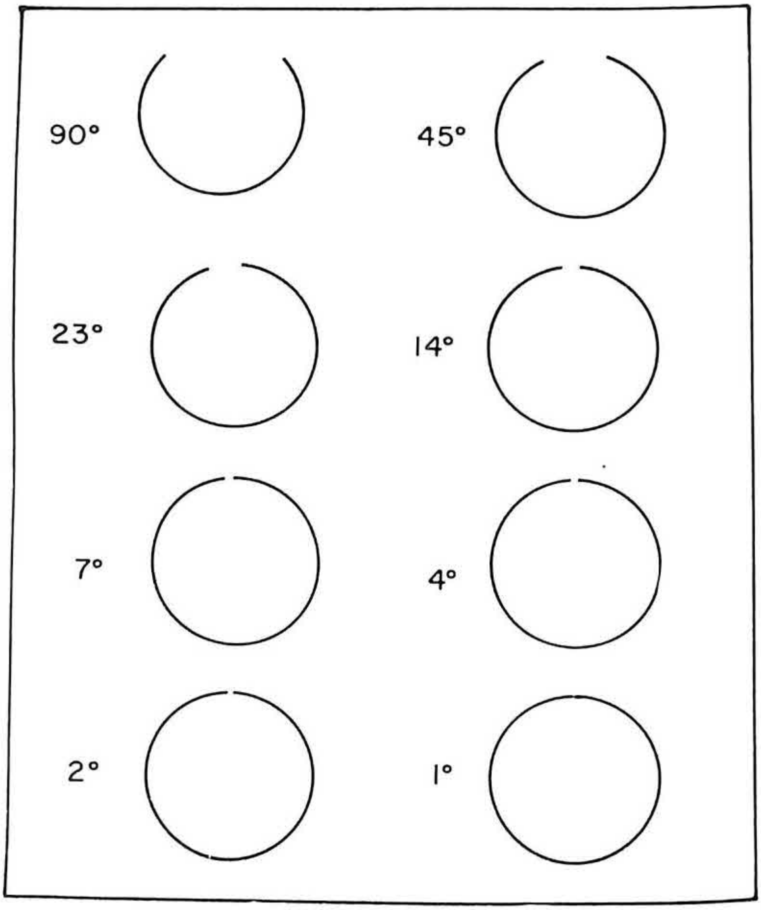

FIGURE 1
POSITIVE STIMULI USED IN THE VISUAL ACUITY TESTING

C. RESULTS

Figure 2 plots the mean errors to criterion for each group on each of the eight problems. The problems are numbered from 1 through 8 in order of de creasing size of separable break in the broken circle.

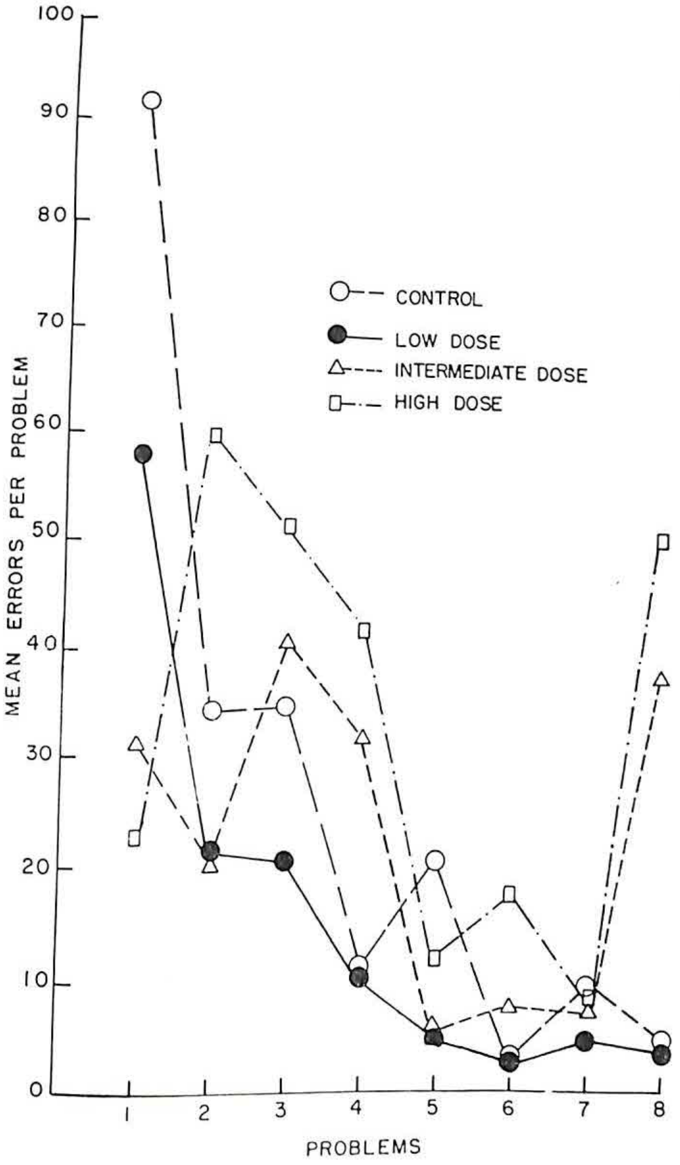

FIGURE 2
MEAN ERRORS TO CRITERION FOR EACH GROUP ON EACH OF THE EIGHT PROBLEMS

Statistical analysis of the data, using a groups by problems analysis of variance design (as shown in Table 1) yielded (*a*) no significant differences between groups, (*b*) a problem effect which was significant beyond the 0.1 per cent confidence level, and (*c*) a groups by problems interaction which was significant beyond the 0.1 per cent confidence level.

TABLE 1
ANALYSIS OF VARIANCE OF ERROR SCORES

Source	df	Mean squares	F	P
Between subjects	28			
Between groups (B)	3	2,659.62	2.93	NS
Error (b)	25	905.50		
Within subjects	203			
Problems (A)	7	7,062.26	14.91	.001
AB	21	1,646.28	3.47	.001
Error (w)	175	473.37		
Total	231			

Since a statistically significant groups by problems interaction was obtained, the difference between groups was tested at each problem level. The design in each case was a simple randomized design. The groups were found to differ significantly beyond the 1 per cent confidence level on Problem 1, the order of efficiency for groups from greatest to least being the *HD*-Group, the *ID*-Group, and *LD*-Group, and the control group. The groups were also found to differ significantly beyond the 1 per cent confidence level on Problems 4, 6, and 8. The order of efficiency for groups from greatest to least on these problems was the *LD*-Group, the control group, the *ID*-Group, and the *HD*-Group.

D. DISCUSSION

The data of the present study are interpreted as reflecting a chronic visual acuity deficit in monkeys subjected to 616 rep of whole-body irradiation. A visual acuity deficit as a latent effect of whole-body irradiation seems suggested in monkeys subjected to 308 rep. An interpretation of the results in terms of a basic deficit in learning ability for the two aforementioned groups of monkeys is contraindicated by the superiority of these two groups on the initial and easiest problem. This initial superiority probably results from the decreased distractibility of animals in these dose groups as reported by McDowell (2).

The data are of particular interest when it is recalled that animals exposed to 308 rep showed no visual acuity deficit during the first year after exposure.

They support the need for longitudinal studies to fully assay the effects of radiation exposure.

E. SUMMARY

Six normal, 10 low-dose irradiated, nine intermediate-dose irradiated, and four high-dose irradiated rhesus monkeys were tested to a criterion of 21 correct responses (24 trials a day) for two successive days on each of eight visual acuity problems presented in order of increasing difficulty. Each problem required the subject to choose between circles and circles with breaks in order to procure a food reward. Previous visual acuity testing had shown a deficit only in the high-dose irradiated group during the first year following the radiation exposure to which the experimental animals had been subjected three years prior to the initiation of the present study. The results of the present study demonstrated that:

1. The visual acuity deficit noted during the first year after exposure in the high-dose irradiated monkeys (616 rep) was still manifested three years after exposure.

2. Monkeys in the intermediate-dose group (308 rep), who had shown no visual acuity deficit during the first year after exposure, manifested such a deficit three years after exposure.

3. An interpretation of the results in terms of a basic deficit in learning ability for the monkeys of these two groups is contraindicated by their superiority over normal and low-dose irradiated monkeys on the initial and easiest problem.

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VISUAL ACUITY PERFORMANCE OF NORMAL AND CHRONIC FOCAL-HEAD IRRADIATED MONKEYS*

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A. Problem

Davis, Elam, and McDowell (1) found evidence of deficit in visual acuity during the first year following radiation exposure for monkeys receiving a total whole-body dosage of 616 rep. During the same period, monkeys exposed to a total whole-body radiation dosage of 308 rep or less manifested no evidence of such a deficit. McDowell and Brown (2) retested the same group of monkeys approximately three years after the animals had been exposed to radiation. They found a persistence of the deficit in visual acuity noted earlier in the monkeys exposed to 616 rep and, also, a manifestation of deficit in animals which had been exposed to 308 rep.

The present study sought to investigate the visual acuity performance of chronic focal-head irradiated monkeys. It sought to determine if the massive radiation dosages to which they had been subjected two years before the initiation of the study had resulted in a visual acuity deficit.

B. Methods

1. *Subjects*

Nine control and six chronic focal-head irradiated rhesus monkeys with nearly identical training histories were used as *S*s in the experiment. The six experimental *S*s had been exposed to focal-head irradiation with X-rays approximately two years before the present study was initiated.

Two of the experimental *S*s had received 3,000 *r* X-ray radiation to the brain anterior to the caudal aspect of the precentral sulcus fissure. Thirty days later they had received the same dosage to the same area of the brain. The other four experimental *S*s had received equivalent dosages, with the same spacing, to an area of the head containing the inferior parietal lobule and posterior aspect of the temporal lobe.

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* Received in the Editorial Office on September 15, 1958.

[1] This work was conducted at the Radiobiological Laboratory of The University of Texas and the United States Air Force, Austin, Texas, and was supported (in part) with funds provided under Contract AF 41(657)-149 and the School of Aviation Medicine, USAF.

2. *Apparatus*

All testing was conducted in a modified version of the Wisconsin General Test Apparatus with the carrying cage serving as the holding cage during testing. The spatial separation between the two foodwells on the stimulus tray was 12 inches.

3. *Procedure*

Eight separate problems were given in order of increasing difficulty, each problem requiring the *S* to choose between circles and circles with breaks in order to procure a food reward. The circles with breaks are shown in Figure 1. On the first and easiest problem a circle was paired against a circle with a 90 degree break. The remaining seven problems, in order of

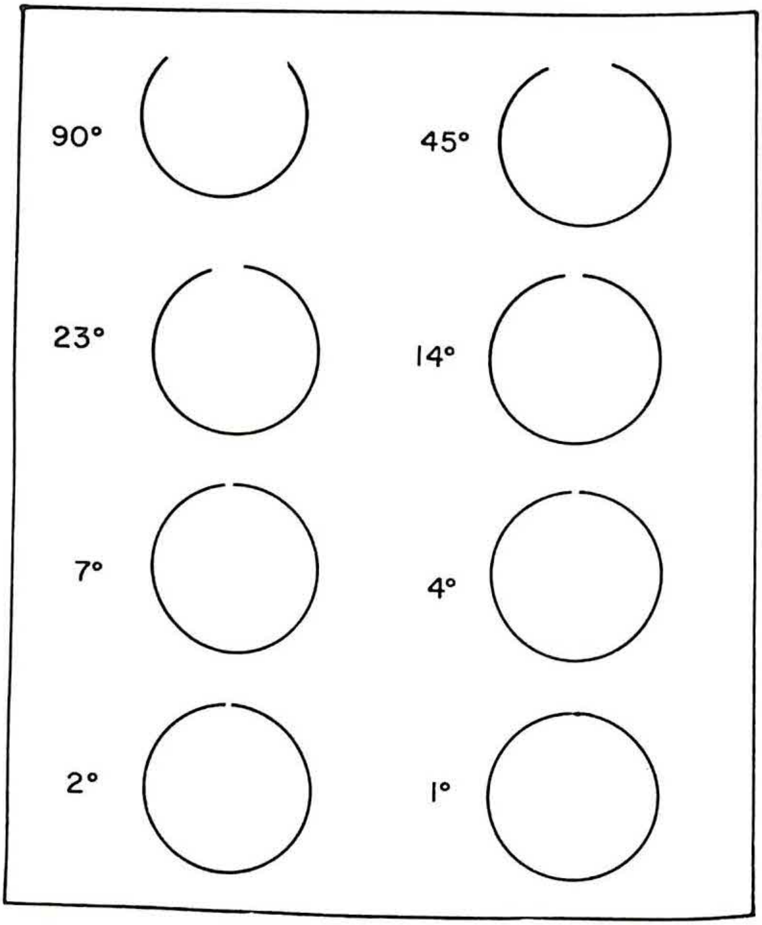

FIGURE 1
Positive Stimuli Used in Visual Acuity Testing

presentation, involved circles with breaks of 45 degrees, 23 degrees, 14 degrees, 7 degrees, 4 degrees, 2 degrees, and 1 degree. Each stimulus card was glued to a 3 3/4 x 4 3/4 inch piece of masonite and was presented in the horizontal plane on discrimination trials. When any control *S* achieved a criterion of 21 correct responses (24 trials a day) for two successive days on a given problem, he progressed to the next most difficult problem. The non-correction method was used.

In order to assure testing on each problem for all of the experimental *S*s, each experimental *S*, if he failed to achieve the above criterion in the number of days taken to criterion by the least proficient normal animal on any specific problem, progressed to the next problem.

C. RESULTS

Figure 2 plots the mean errors per day for each group on each of the eight problems or conditions. The problems are numbered from 1 through 8 in order of decreasing size of separable break in the broken circle.

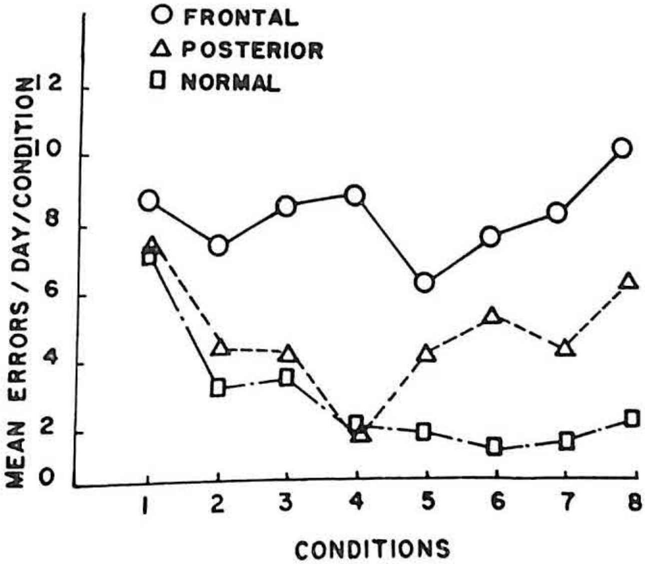

FIGURE 2

MEAN ERRORS PER DAY PER GROUP ON EACH OF THE EIGHT PROBLEMS

Statistical analysis of the data, using a groups by problems analysis of variance design (as shown in Table 1) yielded (*a*) a difference between groups which was significant beyond the 0.1 per cent confidence level and (*b*) a problem effect which was significant beyond the 1.0 per cent confidence level. The groups by problems interaction was not statistically significant.

TABLE 1
ANALYSIS OF VARIANCE OF MEAN ERRORS PER DAY PER PROBLEM

Source	df	Mean squares	F	P
Between subjects	14			
Between groups (B)	2	199.87	12.31	.001
Error (b)	12	16.23		
Within subjects	105			
Conditions (A)	7	32.61	3.05	.01
AB	14	6.65	.62	NS
Error (w)	84	10.69		
Total	119			

D. DISCUSSION

The results of this study reflect differences between chronic focal-head irradiated monkeys and normal monkeys with respect to performance on visual acuity problems requiring the discrimination of breaks in circles. Most efficient performance was shown by the control animals, intermediate efficiency of performance by the animals with previous focal-head irradiation of the posterior association areas, and least proficient performance by the animals with previous focal-head irradiation of the frontal association areas.

Since the animals with focal-head irradiation of the frontal association areas showed no improvement in performance with successive problems, their impaired efficiency may reflect a basic learning deficit with respect to planometric discriminations, a severe deficit in visual acuity, or both factors. Both the control animals and the animals with focal-head irradiation of the posterior association areas showed improvement in performance from Problem 1 through Problem 4. From Problem 5 through Problem 8 the performance of the animals of the control group remained asymptotic, while that of the posterior association area animals became progressively worse. These results suggest, then, a deficit in visual acuity for the animals with previous focal-head irradiation of the posterior association areas.

E. SUMMARY

Nine normal monkeys, four monkeys with previous focal-head irradiation of the posterior association areas, and two monkeys with previous focal-head irradiation of the frontal association areas were tested on each of eight visual acuity problems presented in order of increasing difficulty. Each problem required the subject to choose between circles and circles with breaks in order to procure a food reward. The results of the study demonstrated that:

1. Normal animals show gradual improvement in performance from problem to problem and manifest no loss in efficiency as the most difficult problems are presented.

2. Animals with previous focal-head irradiation of the posterior association areas show gradual improvement in efficiency of performance through the first four problems with a gradual decline in efficiency of performance through the last four problems. These results suggest a visual acuity deficit.

3. Animals with previous focal-head irradiation of the frontal association areas show no improvement from problem to problem. These results suggest either a basic deficit in learning ability with respect to planometric discriminations or a severe deficit in visual acuity, or both factors.

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The Journal of Genetic Psychology, 1960, **96**, 145-164.

CONSTANCY OF CONTENT IN RORSCHACH RESPONSES*

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(With the assistance of Marjean Kremer)

A. INTRODUCTION

Though several longitudinal Rorschach studies (7-10, 12-16) have been reported in the literature, for the most part these have discussed mean score changes rather than changes from year to year in the responses of any given individual subject. Particularly lacking in the literature appear to be studies of yearly change or consistency of content in individual subjects.

The only study which we have found which discusses constancy of individual content over a period of years is Allen's (1) which reports age changes in content in one subject between the ages of 4 and 10 years. This study reports the identification of "three engrams which have appeared consistently throughout the series. While other percepts have been repeated in a scattered pattern, the persistence of these cannot be overlooked."

These engrams consisted of the response "church" to Plate VII (Area D 11, Beck's location, 5, p. 221); and "lightning and smoke" to all of Card II, gradually separated into the separate concepts "lightning" (Area D2) and "elephants" (D1 + D1).

The present study aims to determine whether or not this consistent repetition of some single item of content is more or less unique to the subject reported in Allen's study, or whether it may be a more or less customary phenomenon.

B. SUBJECTS AND METHODOLOGY

Subjects for the present study included a group of 8 girls and 21 boys between the ages of 2 and 10 years, tested at 2, 2½, 3, 3½, 4, 5, 6, 7, 8, 9, and 10 years; and 35 girls and 30 boys between the ages of 10 and 16 years of age, tested annually.

Not every subject was seen at every age, but of 88 possible tests (which we would have had, had every one of the younger girls been seen at every one of the ages used), 71 were available. Of 231 possible younger-boy tests,

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[1] Thanks are due to the James McKeen Cattell Fund for substantial support of this investigation.

185 were given. At the older ages, of 245 possible tests (which we would have had, had every one of the older girls been seen at every one of the ages used), 210 were available. Of 210 possible older-boy tests, 178 were available.

Intelligence of younger subjects was rated on a developmental scale since *IQ* data were not available for all younger subjects. This developmental rating appears in Table 1. Of the older subjects, intelligence quotients were available for 31 of the 35 girls, for 29 of the 30 boys. The mean *IQ* of the girls was 121.06, *SD* 12.51; of the boys, 122.17, *SD* 13.06.

TABLE 1
DEVELOPMENTAL RATINGS, YOUNGER SUBJECTS

Ratings	8 girls	21 boys
Very superior	4	5
Superior	2	7
High Average	1	7
Average	1	1
Low Average	—	1

TABLE 2
SOCIO-ECONOMIC STATUS

Paternal occupation, Class:	Younger group		Older group	
	8 girls	21 boys	35 girls	30 boys
I. Professional	5	16	19	18
II. Semi-professional managerial	2	4	8	7
III. Clerical, skilled trades, retail business	—	1	1	2
IV. Farmers	—	—	—	—
V. Semi-skilled, minor clerical, minor business	1	—	1	1
No data	—	—	6	2

Socio-economic status was rated on the Minnesota Scale for Paternal Occupations. Data appear in Table 2, for both younger and older subjects.

All records were analyzed for consistency of concepts. Records of the 2- to 10-year-olds, and records of the 10- to 16-year-olds were analyzed as two separate groups; as were records of girls and of boys.

A concept was counted as being used consistently over a period of time if an identical response was made to any given card for a minimum of four consecutive age periods. Considerable leeway was given on Card V where a variety of "bird-bat-butterfly" responses were considered as consistent. On other cards, the demands of consistency, as examples will show, were more rigorous.

As Table 5 indicates, many of the instances of consistency found here covered an age range considerably greater than the minimum requirement of four consecutive age levels.

C. Results

Means and medians were first calculated, to determine the average number of cards per child on which consistency of content (same response for a minimum of four consecutive ages) was found. Table 3 gives results, by sexes, for the younger and older age groups.

TABLE 3
NUMBER OF CARDS PER SUBJECT WITH CONSTANT RESPONSE*

	Younger group		Older group	
	Girls	Boys	Girls	Boys
Mean No. of cards constant	3.9	3.9	4.8	5.0
Median No. of cards constant	3.0	4.0	5.0	5.0

* Only one constancy per card is counted even when more than one occurred. However, at the older ages especially, there was frequently more than one consistent concept on any one card. Eleven of the 35 older girls repeated consistently more than one concept on certain cards. One gave from 2-5 consistent concepts each on five different cards. Of the older boys, 15 repeated consistently more than one concept on one or more cards. At the younger ages, only three girls and three boys repeated more than one concept to a card.

Thus it appears that, in both sexes and for both age ranges, whether we calculate medians or means, except for the median of the younger girls, over one-third of the ten cards elicit a constant response over at least four consecutive age levels.

If we figure the total number of possible consistencies against the total number found, we discover that there is consistency of content in 37% of all possible opportunities for the younger girls, 40% of the younger boys, 49% of the older girls, 49% of the older boys. Thus the percentage of all responses which remain consistent increases with age.

1. *Cards Which Elicit the Most Consistent Responses*

Table 4 shows, for each sex and each age range which cards elicit the most consistent responses. As the table shows, for the younger girls Card V stimulates the most consistent responses, Card VIII next most. The least consistency is found on Cards VI and IX. For younger boys, Cards V, VIII, and III stimulate the most consistency, in that order. Least consistency is found on Card IX, followed by I, VI, and X.

With older girls, the most consistency is found on Cards V, I, VII, and X in that order; least on Card IX. With older boys, the most consistency is found on Cards V, III, I, and VI; least on Cards IX, IV, and X.

Thus at younger levels, Cards V, VIII, III, and II obtain the largest number of consistent responses; at the older levels, Cards V, III, and I. At the younger levels Cards VI and IX obtain the smallest number of consistent responses; at the older levels, Card IX falls considerably below the others.

TABLE 4
NUMBER OF CONSISTENT RESPONSES ELICITED BY INDIVIDUAL CARDS

No. consistent responses on card	Younger group 8 Girls	Younger group 21 Boys	Older group 35 Girls	Older group 24 Boys
I.	3	5	20	15
II.	4	10	16	11
III.	2	13	18	18
IV.	2	6	17	7
V.	7	15	24	19
VI.	0	5	13	15
VII.	3	6	19	12
VIII.	5	14	17	10
IX.	1	4	8	5
X.	3	5	19	7

2. *Ages at Which Most Consistency Occurs*

The longest duration for which consistency is found is counted in making this analysis. For the younger girls and also the boys, the most consistency is found for the age ranges 5-10, 6-10, 7-10. For the older girls, by far the most consistency is found for the age range 12-16. Boys are more varied, having an almost equal number of subjects who maintain consistency from 13-16, 12-16, and 10-16 in that order.

3. *Examples of Constancy of Content*

The clearest view of the extent to which constancy of content can be observed, in any individual subject, from age to age, may be obtained from specific examples of such constancy. Several examples for each sex and each age range studied are given in Table 5. We have here avoided the most obvious examples such as "men" on Card III or "bat" on V.

4. *Extent to which Consistent Concepts Are Popular Responses*

A next consideration, having determined that constancy of content from one age to another is not an unusual phenomenon, is to discover to what extent these specific contents which are repeated from age to age consist of mere *popular* responses, and to what extent they are, if not original, at least more infrequent in appearance than the so-called populars.

For the younger age group, the list of populars used was that determined

TABLE 5
EXAMPLES OF CONSTANCY OF CONTENT

Age in years		Response
Younger Girls:		
J.I. — Card V:		
2½	W F + nat.	A rainbow. (Has refused first four cards. Hands on back of chair. Can't point. On second presentation says 'rainbow'. (Either all of card or left-hand side.)
3	W FC′ nat.	A rainbow. (Runs finger around whole shape. Inquiry brings no clarification so she is finally asked, 'What color?') Black.
3½	W F + nat. W F ± A	Rainbow. (?) I saw it. (Eyes Ex. and grins.) (?) Cause I saw it when I tate it. (She points.) (Else?) Caterpillar. (Hands behind her head. Asked 'How much?' says Whole thing.)
4	W F + nat.	A rainbow. (Else?) No. (Refuses all but this and II).
5	W F + nat. W F ± nat.	That looks like a rainbow. (Else?) And another looks like one of those bees.
J.I. — Card VIII:		
3	W CF? nat.	Another rainbow. (All). (What made you think?) (Just points to show "all").
4	—	Nothing.
5	W CF nat. DW FM A	Looks like a rainbow, pink and blue. Or a mountain with a bear going up one side and a bear going up the other.
6	W CF nat. DW FM A P	Rainbow. Bear went over the mountain.
7	W CF nat. D F + A D F + plant D CF geol.	Rainbow. What did I call them? Some kind of animals, I don't know the name. Tree and colored rocks. Don't think it looks so much like anything.
8	W CF nat. D F + A P	That looks like some kind of a rainbow to me. I used to think these were some kind of monsters, some kind of bears or something. (Do you think so now?) Yes.
9	W CF nat.	That looks like a rainbow. Not in a rainbow shape, just the colors.
	D F + A P	That looks like an animal right there—and so does that—but not pink. I don't know why they . . . .
10	W CF nat.	This one I always say looks like a rainbow because it does. Whole thing—not a rainbow like this (describes arc) but just lots of colors.
	D CF plant	And this looks something like a fir tree—just the green part here (top). These look something like bears or
	D F + A P	something—except they're pink of course—that's about all.
Younger Boys:		
T.M. — Card VIII:		
3	D F + A P	That's a bear and that's a bear. Two pink bears. (Else?)
	D F + Ad	What's these? (pink and orange.) Dogs. (Shows parts.) Eyes (inside.) (Profile, as shows mouth at upper corner.)

TABLE 5 (*Continued*)

Age in years		Response
3½	DW $\overset{\nearrow \mathrm{FM}}{\mathrm{CF}}$ A P	Two pink bears climbing a Christmas tree. (How know it was a Christmas tree?) Cause it's pretty. (Else?) Cause I don't like pines. I just like Christmas trees.
4	DW $\overset{\nearrow \mathrm{FM}}{\mathrm{CF}}$ A P	Bears, aren't they? Climbing up a Christmas tree. (Why?) All this and this. (Orange and pink).
5	DW FM A P	This looks like two bears climbing up a tree. Doesn't look like anything else. This (rest) looks like nothing.
6	DW FM A P	Two bears climbing up a tree.
	D CF geol.	And this looks like two colors of rock.
7	DW FM A P	This looks like two bears climbed up a rock and now they're trying to climb a tree.
8	DW FM A P D F ± geol.	Looks like two bears climbing up a pine tree—that's some rocks underneath it. (Rocks?) Looks like rocks and they couldn't be anything else.
9	DW FM A P	Looks like two bears climbing a pine tree.
10	DW FM A P D CF food	It looks like two bears climbing a pine tree. And great big blobs of ice cream they're stepping on to get up there. (?) Looks like pink ice cream—and the bears are made of ice cream too. (?) Just does—the way it's heaped.
R.R. — Card IX:		
4	D C fire	That looks like fire too.
	D k obj.	These look like some airplanes crashed into a tree. Fire or volcanoes.
5	D C fire	Still fire. Whole thing is fire.
6	D C fire	Fire.
7	D CF fire	Flame too. (Pink.) (Remind you?) Wastebasket burning.
	D C grass	This looks like something like grass. (Remind you?) It's green.
8	W CF — A	That looks like fire too. Fiery ears, green body and feet. Whole thing looks like a cat.
Older Girls:		
D. L. — Card X:		
10	D F + flower D FC plant D F + A P	This looks like a flower, so does that. √ And this here looks like sumac. (Else?) These look like either spiders or crabs. (Else?) No. (Idea of flower?) Well they have a stem and there's a flower something like that, all ragged. (Sumac?) Well the sumac is about that shape and that shade of red and it's sort of a brownish red. (Spiders?) Well they have legs here and are sort of rounded.
11	D FC plant D F ± A D F + flower	Ah! My favorite one. All this (pink and gray) looks like sumac. Both shape and color gave me the idea. These look like little crabs (brown and yellow). This all that (blue) looks like scabiosa, the shape.
12	D CF flower D F ± A D FC plant D CF food	My favorite! This looks like a type of an . . . flower. (Blue and green.) Both color and shape. This looks like crabs. This looks like sumac, the red. And this looks like two pieces of popcorn that's gotten all yellow with butter.

TABLE 5 (*Continued*)

Age in years		Response
13	D FC plant D FC flower D F ± A Dd F + arch.	√ That looks like sumac (gray and pink). (?) Color and it looks like that. And these look like flowers (blue only). Color too. And those are crabs (side brown). And that looks like the belfry of a church (nose only of a rabbit head.)
14	DW FC flower	∧ √ This looks like a variety of flowers. The red is sumac; the blue could be—oh, spidery chrysanthemums. Or daisies of some sort.
15	D FC plant D FC flower D F ± A	√ That looks like sumac. (?) Coloring and shape (pink). That looks like a different kind of flower (side blue), can't think of the name. ∧ Those pieces look like crabs (side brown).
16	D FC plant D FC flower D F ± A	√ That reminds me of a sumac. (?) Shape and color. Blue looks like some of my mother's flowers. Spider flowers. Crab (brown and yellow).
D.V. — Card VI:		
10	W F + obj.	God! Looks like . . . seats in a bus with a wall between them. Seats then place to put your feet on. Whole thing.
11	W F + A W F + obj.	Ooh! My goodness! That looks like a bird. Park bench.
12	D F + A W F + obj. D F ± Hd	My goodness, ducks, dead ones. Something to sit on like when you wait for trolley cars. Two rather fat men without any legs. Facing each other. (?) Trolley cars used to have them with seats on both sides, fountain on top.
13	W F + obj. D F + A	Another trolley car bench. A sort of . . . bird . . . an eagle, no, a duck. I don't see anything else.
14	W F + obj. DW FM A	Ah, we've got another trolley car seat, cut the top thing off. That looks like a bird or something flying along the edge of the water, top half is bird, bottom is water. (Much turning)
15	W F + obj. D FM A D F + H	Looks like another trolley car seat. Looks like a bird of some sort (top) flying along in the water (one is the reflection). √ Looks like two fat men without any bottom, I mean without any legs (bottom proj. not included).
16	W F + obj. D F + A D F + H	Looks like a trolley car seat. This thing (usual top) Looks like a bird of some sort with a reflection. √ This way it looks like two fat men. I don't know how they're balanced though. Guess that's all.
Older boys:		
R.A. — Card VI:		
11	—	I wouldn't know about this one. I can't seem to stretch my imagination on this one.
12	W F + obj.	Um! Two halves of a drill or soldering gun. We have one at home—this would be the handle (side proj.) (Halves?) You would have to fold it together.

TABLE 5 (*Continued*)

Age in years		Response
P. B. — Card IX:		
8	DW FC plant	Carrots. Two little animals sitting on top of the carrot bunch, holding a string. There are the carrots, these are the greens.
10, 11, 12 have "animal climbing over rocks."		
13	D FC food D F + (Hd)	Could be carrots and stems. (?) Orange and green and the shape. Could be a horned person—no such thing really.
14	D FC food	√ This way could be two carrots and stems (orange and green). Shape of carrots.
	DW m obj.	∧ Or could be a heavy implement used to grind things and these are the things being pushed up. Handle (midline), crusher on the bottom (pink) and that's splashed up around the edge or forced up (rest of block).
15	D FC food	√ From the other side could be carrots in the ground with their stems (orange plus green). (In ground?) Not in ground.
16	D FC food	Well these look like carrots, these being the green stems.
D. M. — Card II:		
11	D C blood W CF anat. D FC anat. D F + A	Blood—from what I've seen in hygiene it looks like the insides of somebody's stomach, tonsils, red part. (Idea of tonsils?) Reddish. (Stomach?) Red. And this looks like a butterfly. What are these? Ink blots?
12	WS F ± anat.	Insides of somebody's stomach. These would look like his ribs here. And something like that in the pit of the stomach.
13	WS F — anat.	Insides of a person's stomach. (Idea?) Ribs there. (All black spec. curved lines). Pit of stomach (white). I guess these are the lungs (top reds). I don't know much about anatomy. (Ribs?) The roundness, like.
14	WS CF anat.	Looks like the insides of a person's stomach. Looks like the ribs and I don't know. Ribs sorta stick together (all black). In here (center white). Red color. You don't get very appreciative of this after you've seen it for years and years.
15	No exam given	
16	W F(C) anat.	I don't know, looks like insides of human being or animal. (?) These look like either the rib case or lungs, lines going out to the sides. I don't know what this is. (Top red.)

by us (2) in an earlier study. For the older age group we used a modification of Hertz's populars, as listed in our study on adolescent Rorschach responses (3).

In instances where more than one response to a card was repeated over a period of four or more consecutive age levels, if any one of these responses was popular, the response to that card was counted as popular.

TABLE 6
EXTENT TO WHICH CONSISTENT RESPONSES RATED AS POPULAR

	2-10 year old group				10-16 year old group			
	Girls		Boys		Girls		Boys	
	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent
Popular	26	87%	58	70%	111	65%	91	76%
Not popular	4	13%	25	30%	60	35%	28	24%

Table 6 shows the extent to which consistent responses did, and did not, fall among the so-called "popular" responses.

Thus for the earliest age zone, for both sexes, by far the majority of consistent concepts fell among the so-called popular responses. Boys gave relatively more instances of consistency than did girls, but were more original in their consistencies. Not only for the group as a whole, but with individual children, did "popular" responses prevail. Among the younger children all but one girl and all but four boys gave more popular than non-popular responses.

Among the older subjects, also, by far the majority of consistent responses were popular responses. However, in this age group, contrary to our findings with the younger children, boys were less original than girls in their consistent responses. Thus, though both sexes give an increasing number of consistent responses as they grow older, boys' responses are becoming more "popular," less original. In individual older cases, popular consistencies predominate in all but 9 out of 35 girls; in all but 2 of 30 boys.

Examples of consistency of popular response for boys and girls at the two age levels are given in Table 7.

5. *Perseveration*

One special aspect of constancy of response which should be mentioned is that kind of repetition commonly referred to as "magic repetition" or perseveration, occurring in its "static" form chiefly between 3 and 5 years, and in its "dynamic" or changing form, chiefly at 5½ and 7 years.

Perseveration has been defined (2, pp. 109, 156) as repetition of the same response on three or more cards, with the subject giving a certain response to one card and then continuing to call other cards by the same name even though such a designation may be completely inappropriate on the succeeding cards.

By no means every instance of constancy of concept in the early years is an instance of perseveration. However, 75 per cent of our younger girls and 95 per cent of our younger boys do perseverate (see Table 8).

TABLE 7
Examples of Consistency of Popular Response

Age in years	Score	Response
Younger girls:		
A.L. — Card VII:		
3½	D F + H P	Looks like a girl. Two girls. (Tiers I, II).
4	DW F + A P	Two deers on two rocks. Nothing else.
5	DW FM A P	That looks like two bunnies hopping on two stones.
6	D F + A P	These look like two funny little bunnies.
7	DW FM A P	Looks like two rabbits talking together. They're going like this. (Demonstrates turning away, then twisting back over shoulder.) And they're turning their heads. (Bodies are Tier III).
8	W FM A P	Now this looks like two rabbits, bending around looking at each other. (Head a little to one side.) All I can get out of it. (Bottom part is, she says, part of the rabbit but "it comes off.")
9	D FM A P	Oh I know, this looks like two bunny rabbits and they're turning around looking at each other.
	W M H P	Oh yuh here, two girls looking at each other. (Faces are inside profiles of tier 3). Other arm and other leg, don't know where it is. That's all.
10	D FM A P	Oh—this looks like two bunnies looking over backward to see each other.
	W M H P	Oh and this one—you know those dancers with their hair up—this one looks like they're dancing, and turning to see each other. Skirts out here.
Younger boys:		
P.S. — Card II:		
3	W F + A P	That looks like an elephant. (Points on request.) Oh dis (other side) is an elephant too.
4	Cn	Red and black. (Else?)
	W F + A P	Elephant. Baby elephant and mama elephant in circus.
5	W F + A P	Two elephants.
	D F ± A	Two sea lions (top reds).
6	?	(Laughs). Oh this looks like some of those tunnels with a steeple up here (center of bk with only hands).
	?	This looks like a mountain right here. (?) Just looks sort of like a mountain.
	W F + A P	Oh I know something, two elephants.
	D CF obj.	Fireman's hat. (?) Looks like them.
7	W M H	(Sighs) People dancing. Heads and hair sticking up.
	WS F + Ad	Elephant ears. (Head is white, ears are dark).
	D F ± A	Looks like seals (top red).
8	W F+ A P	Two elephants.
	D F+ A	Two butterflies (top red). (No color elicited). One butterfly (bottom red). (?) Looks like swallowtail.
	D F + nat.	They're maple seeds (top red). (i.e. Maple seeds not butterflies.)

TABLE 7 (*Continued*)

Age in years	Score	Response
9	W FM Ad P	This looks like two elephants' heads, trunks going up. (?) Holding trunk together.
	D F + A	This looks like a butterfly—I think that's the same thing I said last year. (Lower red.) (?) Wings around there and some butterflies have that tail.
C. G. — Card VII:		
10	D F + A P	Heads look sort of like rabbits.
	*Dd F + arch.	This looks like a church.
	D FM A	This looks sort of like a curled up kitten. Face and ears made me think.
	D F + A	Body looks sort of like a dog.
	D F + obj.	Third part looks sort of like a bow. Guess that's all.
11	Dd F + arch.	Looks like a church.
	D F + A P	Looks like rabbits.
	Dd FM A	This looks like a camel sitting down. This part is the back and the face.
	D F + A	This looks like a dog.
	Dd F + Ad	This looks like a lion's face.
		(Church?) (This part in here, the white part.) (Rabbit?) This whole thing.
12	D F + A P	Those look like rabbits (tier 1, both sides.)
	D F + Ad	That looks like a cat.
	DF + A	That looks like a dog, a Scottie (tier 1, one side.)
	DF + A	Camel (side view of 1, head and humps).
	Dd F + arch.	That looks like a church.
13	Dd F + arch.	That looks like a church.
	D F + A P	Those look like rabbits (top two tiers).
	Dd F + nat.	Those look like icicles.
	D F + A	That looks like a dog.
	D F ± A	Those look like cats (bottom tiers, facing out).
	D F + H	Those two little things, little white things, look like people.
14	D F + A P	These look like rabbits. (?) Long ears.
	D FM A	And these look like cats. (?) Look like a cat's face and all curled up.
	Dd F + arch.	And this looks like a church. (?) Steeple and door.
15	D F ± A	This looks like a cat (right side of Tier III).
	D F + A P	This looks like a rabbit (Tiers I and II, one side).
	D F + obj.	This part here looks like a mask (Tier II).
	Dd F + arch.	This looks like a church (center bottom).
	Dd F + nat.	√ These look like icicles. (Turns and studies). That's all.
16	Dd F + arch.	This looks like a church (center bottom).
	D F + A P	This looks like a rabbit (top two tiers, one side).
	D FM A	This looks like a cat curled up (half bottom tier).
Older boys:		
B. C. — Card VII:		
10	D $\overset{\overset{m}{\nearrow}}{F(C)}$ nat P	Clouds. Shape of half a person. Hair, wind blowing. (?) Head, neck, body; I don't see the arms, feet or legs.

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* Scored by Hertz Dd. In children under 10 years, we score this Area D.

TABLE 7 (*Continued*)

Age in years	Score	Response
11	DW $\overset{\nearrow \mathrm{m}}{\mathrm{F}}$(C)(H)P	Two angels up in the clouds in the air—two clouds. (Else?) No, there's their hair up in the air, head, cloud, dress.
12	DW $\overset{\nearrow \mathrm{F(C)}}{\mathrm{M}}$ H P	These look like two little children or gods. (?) They look like they're dancing. That's all. (Idea of gods?) This looks like their heads and this looks like a cloud they're on and this looks like braids up in the air.
13	DW $\overset{\nearrow \mathrm{F(C)}}{\mathrm{M}}$ (H) P	This could be two more angels on a cloud. They're talking to each other.
14	DW F(C) H P	Those are two little mermaids or girls on a cloud.
15	W F(C) obj. P	What are those figurines that are sometimes seen up in the clouds?
16	DW $\overset{\nearrow \mathrm{F(C)}}{\mathrm{M}}$ (H) P	Well these are the two little nymphs dancing on a cloud.
R. C. — Card VIII:		
10	DW FM A P reflect.	This could be an animal of some sort crossing over a swamp. This could be the water on the right here and this could be the reflection.
11	DW $\overset{\nearrow \mathrm{CF}}{\mathrm{FM}}$ A P reflect.	Looks to me as though it were a muskrat approaching the water and his reflection in the water. Blue and center part is the water.
12	DW FM A P reflect.	Hm! Most of these could be reflections—same on both sides. > Animal, muskrat, or otter—coming down to the water—reflection in the water. This is the whole thing. The color had nothing to do with the water, but the mirror image gave me the idea.
13	DW FM A P reflect.	Looks like a waterline (midline) here, rocks here and a reflection in here, in the water. This could be a coon or something could be coming down to the water to drink.
14	DW FM A P	∧ ∨ ∧ Looks like an animal approaching the water, with a reflection here. (Water in midline).
15	DW $\overset{\nearrow \mathrm{F(C)}}{\mathrm{FM}}$ A P reflect.	Looks like an animal similar to a muskrat or an otter, approaching a stream. Bank or edge of stream, here reflection—gives a glossy effect. Animals and rocks and whatever may be at the bank of a river.
16	DW FM A P reflect.	This looks like some animal like a raccoon or a muskrat going down to the water (midline) and reflection. (Reflect because mirrored, not shading.)

TABLE 8
PERSEVERATION OF CONCEPTS

Age	Girls No. of subjects available	Girls % who perseverate	Boys No. of subjects available	Boys % who perseverate
2 yrs.	2	50%	6	50%
2½	4	100%	10	70%
3	6	67%	14	50%
3½	6	50%	14	72%
4	7	14%	21	30%
5	8	12%	21	24%
6	8	25%	21	24%
7	8	0	20	0
8	8	0	19	5%
9	7	0	19	5%

6. *Varying Scoring in Constant Concepts*

Though not occurring in a majority of cases, in several instances a subject maintained consistent content from age to age, but with varying scoring which to some extent bore out the kind of age changes characteristic of the different age levels (3). Most commonly, movement drops out at or around 13 years and sometimes at 15 (Table 9).

7. *Awareness of Repetition*

There appears to be very little awareness, or at least little mention, of giving the same concept as at an earlier age, or even of having seen the same card before, in the first nine years of life. Even at the older ages, many subjects go on year after year giving the same response either with no comment as to having given it before, or even with enthusiastic remarks as to their own remarkable imaginativeness and ingenuity: "Bet you never heard this before!"

However, from 10 years on increasingly through 15, with a slight dropping off at 16, a substantial number of subjects (as many as 1/5 of subjects at some ages) do comment either on the sameness of the cards or on the sameness of their own response. Girls appear to be a little more aware of or at least more verbal about the fact that the cards, or their responses, are the same from year to year.

Boys are a little more likely than girls to be annoyed about or to comment adversely about this sameness: "You don't get very appreciative of this after you've seen it years and years"; "Same old thing I always see. Gets tiresome!" Most boys, however, merely indicate the fact that they have made the response before: "I remember this one"; "Still looks like a coyote skin"; "OK to say the same thing as before?" "I still get Paul Revere's Ride";

TABLE 9
VARYING SCORES IN CONSTANT CONCEPTS

Girl M.B. — Card I:		
10 years	W M (H)	I am going to have it still looks like two witches. (Idea?) They're stirring in a big pot. This part looks like witches. This part of their arms and hat, and their neck.
11 years	DW M (H)	I do remember that one. I'm going to change it this time. I always have it the same. Guess I'm not going to change it. No, it's two witches on two sides and then in the middle they're tearing apart a girl and taking her head off.
12 years	DW F+ (H)	It always looks like the same thing. (Much turning.) It looks like I guess two witches with somebody's head cut off in the middle. (Else?) No.
13 years	DW F+ (H)	Always this! Two witches with a maiden with her head cut off. (Else?) All! Should it look like something else? (Up to you.) Nothing.
14 years	DW M (H)	Oh! A witch with a girl with her head cut off in the middle—two witches. (Tell about?) They're holding her.
15 years	DW $\overset{\nearrow \text{FClob}}{\text{M}}$ (H)	It's two witches holding a girl; dead girl without a head.
16 years	DW M (H)	It looks like two witches holding a girl with her head cut off.
Boy R.A. — Card I:		
11 years	W M H	Uh! They look like an acrobat using two bears, one on each side, sort of bracing themselves in the middle object. (Where?) Here, sort of hands spread out, twirling around. There's two of them. (The bears are the acrobats.) (Middle?) Sort of looks like a headless woman with hands sticking up in the air.
12 years	DW M H	Uh—let's see! Oh, looks like a lady with two bears on the outside—looks like an acrobat with two bears. (?) Sorta lady with a bear on each side.
13 years	D F+ A D F+ H	Oh! Two bears. And a lady in the center. (Else?) No. (Idea?) Two small feet, skirt, I don't know, two hands.
14 years	DW FM A	Well, two bears and a girl in the center—guess that wraps that up. (?) Bears sort of reach out from her.
15 years	D F+ H D F+ A	Sure I'm always saying the same thing each time. A woman (center). Bears (sides).
16 years	DW M H	Well, uh, looks like two bears and a lady dancer. (?) Uh, it could be associated, all in one picture. Whirling around, lady is. Unethical but . . . .

"Looks like my (same) soldering iron."

Girls, considerably more than boys, seem to feel apologetic or even guilty about giving the same response from age to age: "You know, I can remember what I say about these" (Looks guilty); "I said that last year but it still looks like it." Quite a number of girls, and some boys, express a resolve, not always carried out, to think of something different: "Wish I could think of something different," "I'll see if I can't get something more sensible than I did last year," "I do remember that one. I'm going to change it this time. I always have it the same. Guess I'm not going to change it. No, it's two witches on two sides and then in the middle they're tearing apart a girl and taking her head off."

8. *Individuality as Revealed by Constancy of Content*

Though with our older subjects, girls and boys alike give both a mean and a median of five cards with constant responses over a period of four or more years between 10 and 16, the range in girls is from 0 to 8, for boys 1 to 8. Thus, obviously some subjects are much more constant in the content of their responses than are others.

Analysis of the nature of these constant responses suggests that three classifications of subjects can be made subjectively. In the first group are those girls and boys who have a high number of consistencies (six or more per child) but whose consistent responses are somewhat stereotyped and unimaginative.

A second group of subjects have very few consistencies—three or less per subject.

The third group is made up of those who again have many consistencies—six or more per child, but whose responses are detailed, imaginative, and of seemingly richer quality than are those in the other two groups.

Quality here is judged *subjectively*. However, in an attempt to quantify our judgments in Rorschach terms, we determined mean M, mean sC, and mean F% for each of the three groups just described, girls and boys separately. As Table 10 shows, there is a very clearcut difference between Groups I and III, subjects in Group III having on the average a markedly higher M, in the girls a higher sC, and in both sexes a lower F%.

There is some suggestion in Table 10 that our Group II, that is the subjects who gave the least consistent responses, may be of a lower general quality than Group I, but our clinical judgment (both of the responses and of the subjects themselves) is that subjects in Group II are superior in intelligence and in general quality of life responses to those in Group I.

TABLE 10
INDIVIDUALITY REVEALED BY CONSTANCY OF RESPONSES

Number of subjects		Mean No. of cards with constant P*	12 years			14 years		
			Mean M	Mean sC	Mean F%	Mean M	Mean sC	Mean F%
				Girls				
Group I	5	6.8	2.6M :	1.0sC	65.80%	2.0M :	.7sC	69%
Group II	5	2.0	1.8M :	.9sC	70.75%	1.0M :	1.3sC	71%
Group III	6	7.5	3.6M :	2.1sC	54.00%	4.0M :	2.0sC	52.6%
				Boys				
Group I	5	6.4	1.4M :	1.7sC	64%	1.4M :	1.2sC	65%
Group II	5	2.2	2.3M :	1.5sC	40%	2.0M :	.7sC	60.8%
Group III	6	7.1	3.4M :	1.0sC	48%	4.0M :	.7sC	45.7%

* As elsewhere in this paper, a constant response is a response to any given card which remains the same for at least four successive ages.

If our observations, which admittedly are somewhat subjective, are correct, repetition of any given response from one age to another occurs in two kinds of subjects—those with less than the average enlivenment in their Rorschach response (Group I) and in those with considerably above the average enlivenment (Group III). That is, some subjects repeat a popular or unenlivened response from age to age more or less automatically, expressing little of a unique, original individuality. A second group of subjects, also marked by outstanding consistency of response, includes some of our most superior subjects from the point of view that they give an enlivened response. We get the impression that these subjects, by the time they reach the 10-16 year old range, express a solid core of individuality which does not change markedly in its expression from age to age even though the response is greatly enlivened.

9. *Sex Differences*

Sex differences are not conspicuous. The mean number of consistent responses for the younger subjects and the medians for older subjects are identical for girls and boys. Boys have a slightly larger median than girls in the younger group, and a slightly larger mean than girls in the older group. Thus, younger girls are consistent in 37 per cent of all possible instances; younger boys in 40 per cent. Of older subjects, both boys and girls are consistent in 49 per cent of all possible instances. Thus the very slight difference in frequency of consistency is in favor of the boys.

As to the cards which elicit the most and the least consistent responses, sex differences are negligible.

As to popularity of consistent responses, though younger boys do give relatively more instances of consistency than do girls, they have fewer popular consistencies: 87 per cent of consistent responses in the younger girls are popular; only 70 per cent of boys' consistencies. Among older subjects, however, only 65 per cent of girls' consistencies are popular, 76 per cent of boys'. Thus girls give fewer popular consistent responses as they grow older; boys give more.

Girls appear to be a little more aware, at least verbally more aware than are boys, of seeing the same card from age to age, and/or of giving the same responses. Girls more than boys try not to give the same response, or seem to feel apologetic about repetition. Boys, if they show any emotion about repetition, seem to feel annoyed or bored by it.

D. Discussion

Allen (1) suggests, and we agree, that there does not seem to be in most cases any immediate relationship between a child's current life situation and the nature of repeated percepts. However, Allen feels that the question, "Why are certain engrams repeated in record after record?" *obstinately calls for an answer.*

Of the three responses reported on his one subject—*church* on Card VII, and *lightning* and *elephants* on Card II—the author cannot to his satisfaction explain the source of the first, i.e., *church.* Of the others, he concludes that "the first lightning response has its probable origin in the child's vivid experience of an unusually severe hurricane (with brilliant lightning bolts and thunderous thunderclaps) two days prior to testing. Prior to the 5 year 3 month test, the child was given the explanation of the phenomenon of a tropical lightning and thunder storm. "The violence of the response was markedly reduced" and *thunder* became a less anxious concept, a favorite child's circus animal—the *elephant.*

Allen concludes: "The essential issue involved . . . is the possibility of misinterpretation inherent in the evaluation of a single protocol. Moreover, inadequate information about a testee's background may result in inappropriate inferences, especially in regard to the deviant engrams. From a realistic and practical point of view, the dilemma does not yield to easy solution . . . This does impose upon the test user the responsibility of avoiding blind interpretations."

It does, of course, seem obvious that a concept must be in a subject's repertoire before he can use it. However, in the case of Allen's subject, one of the three consistent responses observed, *elephant,* on Card II, is a popular response, and one which quite normally comes in following earlier, less modulated responses to this card such as *fire,* tree. The *church* response on VII, though not a popular response, is also a common one with young children.

In view of the fact that consistency of response is found as prevalently as in the present study, neither the content of the responses given by Allen's subject, nor the fact that they are repeated from age to age, seems surprising. And though blind analysis inevitably has its disadvantages, nothing in the present study indicates any special need of intimate knowledge of a subject's life and activities in order to make proper interpretation.

E. Summary

Content of Rorschach responses of 8 girls and 21 boys tested annually from 2 to 10 years, and of 35 girls and 30 boys tested annually from 10 to 16 years (not every subject available at every age), was analyzed to determine the extent to which individual subjects gave identical responses from age to age.

It was found that both girls and boys in the younger group gave consistent responses (same content on any one card on four or more consecutive tests) on a mean of 3.9 cards per subject. At older ages, means for constant responses per subject were 4.8 for girls, 5.0 for boys. Thus the number of cards on which consistent responses are given increases with age.

By far the majority of these consistent concepts fall among the so-called popular (or banal) responses: 87 per cent of younger girls' responses, 70 per cent of younger boys', 65 per cent of older girls', 76 per cent of older boys' fall into the popular category. Thus consistent responses of girls become less popular as they grow older, those of boys more popular.

Frequently, it was observed that though content of response, in any one child, remained the same from age to age, the actual scoring might change, in keeping with the greater restriction or greater enlivenment of the age in question. (Thus movement might drop out at 13 or 15 years.)

Sex differences were small. Younger boys showed a little more consistency than did girls. There was a little difference at the older ages. As indicated, girls' consistent responses became less popular with increased age, those of boys more popular.

Two groups of subjects giving consistent content were noted. One group appeared to repeat a popular or unenlivened response from age to age more or less automatically, expressing little of unique or original individuality. The second group, also highly consistent in their response, included some of our most superior subjects from the point of view of enlivenment.

It is commonly believed that as the individual matures, his Rorschach response settles down and that age changes in this response become less marked from year to year. Findings of the present study that the number of responses per subject which remain constant from year to year increases with increasing age of the subject are in accordance with this general belief. This trend occurs in both girls and boys.

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A REVISED CONCEPTION OF MASCULINE-FEMININE DIFFERENCES IN PLAY ACTIVITIES*

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A. THE PROBLEM

It is commonly assumed that children's games are one of the ways in which boys and girls learn appropriate sex rôle behavior in their own society (1, 2, 7). It follows that differences in the games chosen by boys and girls will reflect their perceptions of behaviors appropriate to their own sex. The common participation of most children in games makes check list questionnaires a simple and direct technique for investigating these differences in perception. This technique has been used in a number of authoritative studies during the past 30 years (4, 7), and has become one of the standard bases for deriving masculine-feminine distinctions in developmental psychology (9).

There is considerable empirical and theoretical consensus that sex rôles in American society are in a fluid state (1). In particular, it appears that the more active part being played by women in modern society is accompanied by an expansion of their rôle perception (3, 5). The authors believed that a re-examination of some of the commonly found male-female differences in games should be undertaken. It seemed likely that distinctions established years ago might no longer be valid. To this end, the present study sought to examine the game preferences of children.

B. PROCEDURE

The immediate task was to compile a representative pool of game items, commonly known and played by children. Children in the grades one through eight were sampled in five midwestern townships ($N=402$). The subjects were instructed to enumerate all the games that they played at home or school. This resulted in a pool of several hundred items mentioned by the children.[2] Games known to very few were eliminated. The game lists of

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[2] Construction of this list was made possible through the kind assistance of Mr. Mert Galbraith, Miss Mae Pike, Mrs. Marian Segrist, Mrs. Victoria Portman, Mrs. Andy Slaninka, and Principals N. Jacobs, H. Lehtomaa, and R. Knestrict and their respective staffs.

Terman (8) and Lehman and Witty (4) were also used as a source of items. The final list of 181 games may be broken down as follows: there are 125 games with formal rules (e.g., tag, London bridges, baseball); 38 general activities and pastimes (e.g., climbing, see-saw, sewing); and 18 dramatic plays (e.g., cowboys, houses). The final list, although twice the size of the Terman list, was similar in composition to it. It was less like the Lehman and Witty list, half of which is composed of pastimes, entertainments, and interests.

The list of 181 games was administered to a sample of 187 children in the fourth, fifth, and sixth grades with the following instructions:

> Here are lots of games. *Mark only the games you have played yourself.* If you liked this game, put an *X* on the *L* which follows it. If you did not like this game, put an *X* on the *D* which follows it. Thus, each game you mark will be followed by *X* on an *L,* or *D,* or *L,* or *D* crossed with *X.* Games you have not played will not be marked.

As the sample was a relatively small one, it was previously decided to limit analysis to those items to which 50 per cent or more of the children responded. This resulted in the loss of 66 items as untestable, and left a list of 115 items to which at least 94 children had responded, and for which an analysis in terms of male-female differences seemed feasible.

C. Results

An item analysis was conducted on the basis of sex and preference. This yielded 18 items chosen more frequently by boys than girls ($P = .05$, or less), and 40 items chosen more frequently by girls than boys ($P = .05$, or less). Of the total 181 items in the list, 57 met the criterion of frequency, but did not differentiate between the sexes. Table 1 presents the discriminating items for boys and girls. Table 2 presents the items which approached, but did not achieve significance. Increasing the size of the sample might well result in significance for the latter, as they are of the order of the discriminating items. The remaining 39 items which failed to differentiate between the sexes (neutral) are presented in Table 3.

The results suggest that boys have fewer games that differentiate them from girls than do girls have games that differentiate them from boys. It is interesting to note that many games traditionally associated with boys (e.g., baseball, soccer, camping) do not significantly differentiate between the sexes in the present sample.

It is difficult to make an adequate comparison with previous studies because of the different methods used by Terman and the lack of complete documentation on the part of Lehman and Witty. Comparison with Terman leads to results similar to those above. Some of Terman's items were omitted from the present list because they are no longer familiar to children (backgammon, crokinole), and some other items of Terman included in the present list did not reach the 50 per cent criterion level. Contrasting the remaining 57 items which the two lists have in common, the following comparison is permitted: of the 27 items that Terman lists as being played predominantly by boys,

TABLE 1
GAME PREFERENCES DIFFERENTIATING BOYS AND GIRLS IN THE FOURTH, FIFTH, AND SIXTH GRADES ($P = < .05$)

Boy	Girl
Bandits	Blind man's buff
Bows and arrows	Building snowmen
Boxing	Cartwheels
Building forts	Clue
Cars	Cooking
Cops and robbers	Crack the whip
Darts	Dance
Football	Doctors
Hunt	Dolls
Marbles	Dressing up
Make model airplanes	Drop the handkerchief
Shooting	Farmer in the dell
Soldiers	Follow the leader
Spacemen	Fox and geese
Throw snowballs	Hide the thimble
Toy trains	Hopscotch
Use tools	Houses
Wrestling	Huckle buckle beanstalk
	In and out the window
	I've got a secret
	Jacks
	Jump rope
	Leap frog
	London bridges
	Mother, may I
	Mulberry bush
	Musical chairs
	Name that tune
	Pick up sticks
	Puzzles
	Red rover
	Ring around the rosy
	Scrapbook making
	See saw
	Sewing
	School
	Simon says "thumbs up"
	Statues
	Stoop tag
	Store

only eight continue to differentiate significantly between the sexes, 17 no longer differentiate, and two differentiate in favor of girls (snap the whip, leap frog); of the seven items common to both scales which Terman states as not differentiating between the sexes, four are still neutral, and three now differentiate in favor of the girls (red rover, follow the leader, fox and geese). Common to both lists are 23 games which Terman found to be predominantly played by girls. Nineteen still differentiate in their favor, and four have become neutral. In summary, there is an apparent shift in the two lists in the direction of increasing preference by females for what were formerly male items.

TABLE 2
GAME PREFERENCES FAILING TO DIFFERENTIATE BOYS AND GIRLS IN THE FOURTH, FIFTH, AND SIXTH GRADES ($P = > .05 < .20$)

Boys	Girls
Basketball	Clay modeling
Bowling	Draw or paint
Cowboys	Gardening
King of the mountain	Hide and seek
Racing	I spy
Soccer	Monopoly
Tug-o-war	Scrabble
Wall dodge ball	Spin the bottle
	Tag
	Tail on the donkey

TABLE 3
GAME PREFERENCES FAILING TO DIFFERENTIATE BOYS AND GIRLS IN THE FOURTH, FIFTH, AND SIXTH GRADES ($P = > .20$)

Walk on stilts
Dominoes
Shuffleboard
Baseball
Boating
Fish
Camping
Climbing
Ball tag
Pool
Post office
Chess
Seven up

Fly kite
Ghosts
Black magic
Dodgeball
Tiddle di winks
Cards
Checkers
Ping pong
Horse shoes
Tennis
Skating
Horse riding
Bicycle riding

Make collections
Hiking
Raise pets
Stunts in gym
Swimming
Horses
Wood tag
Kick dodge
Bingo
Tic tac toe
Dog and bone
Volleyball
Roller skating

It should be mentioned that an analysis of the list provided by Lehman and Witty and a comparison of the overlapping items led to essentially similar results. In that comparison, the number of male items dwindled, neutral items (i.e., lack of significance between male-female preferences) increased, and female items remained more or less constant.

D. Discussion

A number of facts emerge from the present investigation which are worthy of note. First, boys tend to name (or select) fewer items which significantly differentiate them from girls. This finding is consonant with present day theories which emphasize the increasing masculinity of the feminine self-concept (1). However, traditional conceptions of boy-girl differences and Terman's investigation did not prepare the authors for the finding that many of the games typically seen as masculine (baseball, basketball) would not differentiate significantly between the sexes at this early age level. It should be remembered that this difference is based on check-list choice, not actual participation. This difference may partly be accounted for by the fact that females check more items than males in our sample (the ratio of 7:6). Thus, it may well be that the play *practices* of the sexes continue to vary more than the present investigation suggests; for despite their encroachment upon boys' games, girls continue to play and show interest in their own distinctive games. It is highly improbable that emphasis upon traditional girl-games would continue if the girls spent as much time as the boys on the games that the two sexes share.

In light of the present findings, it seems probable that an extension of the female rôle perception is occurring in games. The masculine rôle appears to have become confined, yielding fewer, widely acknowledged ways of seeing the self. It has been observed elsewhere that although modern boys spend as much or more time at their play than did their predecessors, the variety of their games has been considerably reduced (6).

Brown suggests that there are signs of convergence of the two sex rôles in our society (1). The present limited investigation supports only the conclusion that the girls now show greater preference for boy play rôles, not that boys show any greater preference for girl play rôles. It shows, in addition, that the girls retain their own distinctive play rôles. The change appears not to be one of rôle convergence, but of expansion of female rôle perception and contraction of male rôle perception.

In conclusion, the results of the present investigation suggest that to continue to rely upon play data which was accrued several decades ago as the basis for present prediction is highly questionable in view of the marked shift in masculine and feminine preferences. We cannot avoid the suggestion that a widespread sampling of games is the only likely means whereby an adequate comparison of the play-dimension of masculinity-femininity can be entertained.

E. SUMMARY

The present investigation sought to compare male and female differences in game play activities. A check list yielded 18 items differentiating boys from girls and 40 items differentiating girls from boys. Contrasts with traditional conceptions and Terman's 1926 investigation indicated that girls now evidence greater interest in male activities than was formerly the case. This article, therefore, presents further evidence consistent with theories which emphasize the increasing masculinity of the feminine self-concept.

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SPECTROGRAPHIC INVESTIGATION OF INFANT VOWELS*

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A. Introduction

In a recently published article, Lynip (31) criticized the method of recording infant sounds employed in the investigations of Irwin and his colleagues (1, 2, 3, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 26, 27, 38, 39, 40, 41, 42). Irwin and his associates had recorded the spontaneous speech sounds of 95 white infants during the first two and one-half years of life. Their procedure was to transcribe into the symbols of the International Phonetic Alphabet the speech sounds by an infant on the exhalation phase of the breath cycle. This sampling procedure was found to have high observer agreement (12).

Lynip, challenging Irwin's procedure, stated, "Before adequate study can be given the pre-speech utterances of infants, two conditions must exist: (*a*) the gathering of the data must be devoid of such handicaps as the fallibilities of the human ear and of the use of phonetic systems, (*b*) these data must be analyzed in an objective manner" (31, p. 227).

In an attempt to satisfy these conditions, Lynip proceeded to tape-record the vocal utterances of one infant from birth to 56 weeks of age. The sounds, which were recorded in the infant's home, were acoustically analyzed with a sound spectrograph.[2] Lynip examined the obtained spectrograms and concluded as follows:

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[2] As described by Koenig, Dunn and Lacy, "the sound spectrograph is a wave analyzer which produces a permanent visual record showing the distribution of energy in both frequency and time" (30, p. 19). The permanent visual record is referred to as a spectrogram. Kersta adds, "Spectrograms as normally produced by the 'sound spectrograph' are three dimensional. Time is portrayed along the length of the spectrogram paper, frequency across it, and amplitude by the blackness of the marking" (28, p. 796).

A formant is defined as a frequency region of energy concentration in the spectrum of a vowel sound. In the past, the acoustical characteristics of vowels have been graphically represented as two-dimensional plottings of the frequency of Formant 1 *versus* the frequency of Formant 2.

> . . . This infant . . . did not produce a single vowel or consonant sound that was comparable to adult vowels and consonants until about the end of her first year and . . . even at that time the sound was only approximately the equivalent of an adult sound. This study and this method show that any vowel utterance may better be expressed in units of measurement more precise than the generalized forms of phonetic symbols. Because evidence herein submitted shows that one or many syllables of this infant's pre-speech utterances are essentially incomparable to adult sounds, it follows that any record that represents such sounds as adult speech may be therein unreliable. Such inaccuracy of representation was inevitable as long as reliance was put on auditory senses (31, p. 246).

Elsewhere, Lynip states:

> The import of this research on the general field of literature is . . . to show the necessity and possibility for by-passing any use of phonetic symbols and any complete dependence on the hearing sense for analyzing pre-language utterances (31, p. 248).

Lynip's research has elicited the following comments from various investigators. Pratt states, "by these means [the sound spectrograph] the subjective appraisal of sounds which is involved in phonetic recordings by observers is avoided. The new technique makes possible accurate, quantitative samplings of the vocalization of the child within the home environment" (37, pp. 263-264). Harris remarks that Lynip's study of infant sounds is "quite devastating to studies such as Irwin's . . ." (9, p. 11).

The purposes of this paper are: (*a*) to examine the validity of Lynip's assumptions and conclusions, and (*b*) to present a preliminary spectrographic analysis of a small number of infant vowels.

B. ACOUSTICAL AND PHONETIC CONSIDERATIONS

Lynip's position might be summarized as follows: phonetic transcriptions do not provide valid and objective representations of the prelinguistic utterance of infants. Lynip's thesis, which requires the elimination of the human observer from any objective analysis of infant speech, is philosophically untenable. It demonstrates, on the part of Lynip, a confusion of the physical and psychological parameters of sound. As mentioned previously (see Footnote 2) the sound spectrograph is an instrument which measures certain *physical* characteristics of sound, such as intensity, duration, and frequency. On the other hand, phonetic transcription is a tool of *linguistic* analysis, requiring the identification of phonetic elements by the human ear. Whether there is a necessary and consistent relationship between these two sets of measurements is an empirical question that can only be settled by adequate investigation, but is certainly not to be naïvely taken for granted. Investigators interested in the phonetic qualities of infant sounds are by definition restricted to phonetic impressions. Physical measurements of infant sounds cannot be made to represent phonetic transcriptions, unless shown to be perfectly correlated.

Lynip's position that only data recorded and represented by physical means are objective cannot be taken seriously. Observations, however obtained, require some kind of a discriminal response from a person whether it be tactile, auditory, or visual. However, Lynip seems to imply that the only kind of observation that has any validity is an "objective" observation, resulting from such things as needle pointing on dials, or tracing on graphic recorders, etc. Following this line, one would eliminate as invalid: visual and tactile observations by physicians, responses of listeners to a singer's pitch level, a policeman's visual impression of the speed of a car, a speech pathologist's judgment of mis-articulated sounds, and a gourmet's response to food.

As a matter of fact, a certain degree of subjectivity is present in all interpretations placed on spectrographic records, i.e., observers decide by visual inspection the regions of energy concentration in the spectrum of a vowel sound. In psychological research "objectivity" is often measured by a coefficient of agreement among observers. With regard to phonetic transcriptions of infant speech, Irwin and his colleagues have repeatedly demonstrated that the speech of infants can be transcribed phonetically with high agreement between observers (12, 24, 27, 38).

Moreover, the accuracy with which data are interpreted depends not only upon the inherent smallness of error in the record (such as, spectrograms, oscillograms, etc.), but on the correctness of interpretation. The correctness of interpretation depends on the knowledge and experience of the interpreter. If he lacks knowledge of the proper inferences to be drawn from physical measurements of sound, the interpretations can be as erroneous as the phonetic symbols recorded by an observer attempting to make a phonetic analysis without proper training and experience in analyzing what he hears.

Lynip does not seem to recognize that the "validity of these objective indications" (spectrograms) is an empirical question which can only be answered by comparing and correlating the evidence thus obtained to the evidence resulting from some other, more direct and more basic, method of observation. The basic data against which any instrumental method of phonetic analysis must be validated are the phonetic analyses made by competent observers whose validity Lynip questions. Rather than discarding, as Lynip suggests, observations which disagree with his particular (subjective) interpretations of his spectrographic records, it would seem far more logical and appropriate to conclude that the spectrograph may not, in the present state of knowledge, provide appropriate and interpretable evidence on which to arrive at conclusions about speech sounds. Ample support for this statement can be found in the relevant literature of experimental phonetics and will be discussed briefly.

At present, despite the rather extensive research conducted with the sound spectrograph, a great deal of doubt remains as to the essential physical properties of vowel sounds. Research has shown that physical comparisons of phonetically identical vowels are often found to be dissimilar while physical comparisons of phonetically different vowels are sometimes found to be similar. In brief, spectrographic research of adult and children's vowels has indicated: (*a*) considerable overlap among formant frequencies of phonetically different vowels and (*b*) substantial variation of the formant regions of phonetically identical vowels.

Peterson and Barney (34) plotted (Figure 1) the frequency of the first formant *versus* the frequency of the second formant for vowels on which 70 adult listeners unanimously agreed concerning the phonetic value of the vowel actually spoken. The authors conclude that "the distributions of measured formant values in these plots do not correspond closely to the distributions of phonetic values" (34, p. 183). That is, the characteristics which the sound spectrograph will reveal for a given vowel, such as [*a*], may differ considerably from one individual to another.

These differences are magnified when one compares vowels, phonetically the same, produced by men, women, and children. Kock (29), plotting (Figure 2) the frequency of Formant 1 *versus* the frequency of Formant 2 for vowels uttered by an adult male, an adult female, and a child, noted that the "acoustic vowel triangle" (or plot) for the female adult in relation to the male adult was displaced upward and to the right. The child's "triangle" was further displaced. Crandall (6), Chiba and Kajiyama (4), Potter and Peterson (35), Potter and Steinberg (36), Peterson (33), and Peterson and Barney (34) have also shown that the formant positions for a given phoneme vary considerably and systematically for men, women, and children. With regard to this phenomenon Potter and Peterson comment:

> It is apparent . . . that the boundary positions depend upon the size of speech cavities. As might be expected, the diagram is shifted upward and outward in going from adult male to female voices and is shifted still further for the children (35, p. 531).

It may be observed from Figure 2 that vowels identified by listeners to be phonetically the same for a man, a woman, and a child are represented by quite different frequency regions or formants. Thus, one would expect physical differences to appear in a spectrographic analysis of a vowel spoken by a man, a woman, or a child, and identified by listeners as phonetically identical. It follows that even greater differences (in formant frequencies of phonetically identical vowels) would be expected between the vowels of adults and those of infants. If Lynip had been reasonably familiar with the relevant literature concerned with speech sound analysis (4, 6, 35), he would not have expected an infant to "produce a single vowel comparable to that of an adult."

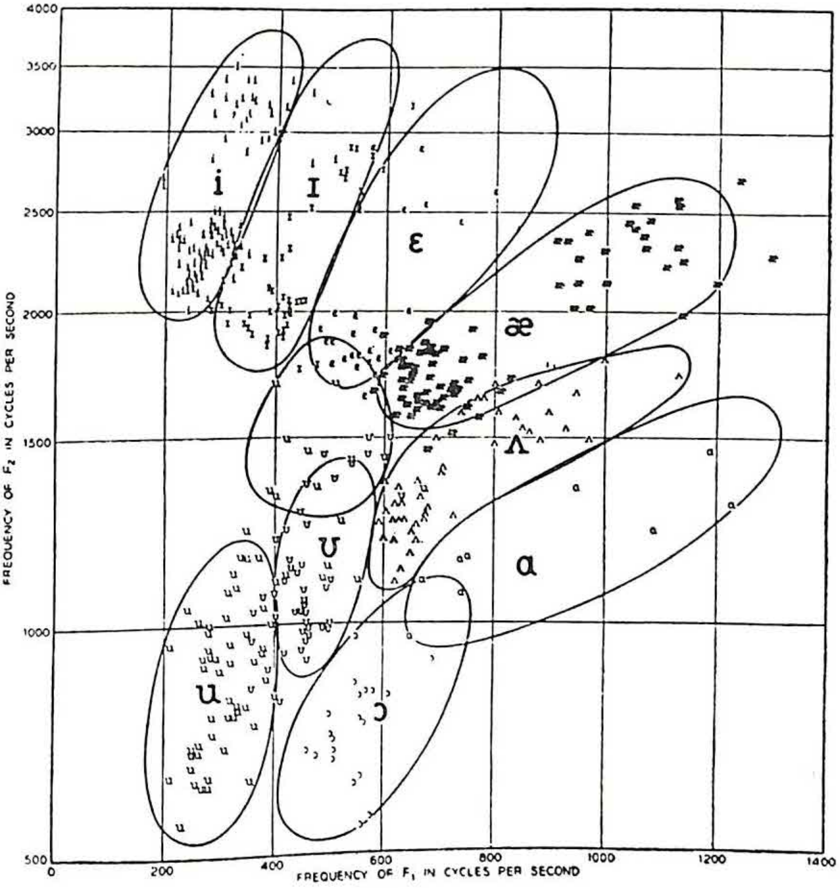

FIGURE 1

Frequency of second formant *versus* frequency of first formant for vowels spoken by men and children, which were classified unanimously by all listeners. (Reprinted with permission of the authors, 34.)

With regard to consonant sounds there is evidence (5) to indicate that the same regions of energy may be heard as different consonants when adjacent to different vowels. Recently investigators (5) at the Haskins Laboratories have been able to synthesize sounds with a pattern playback machine.[3]

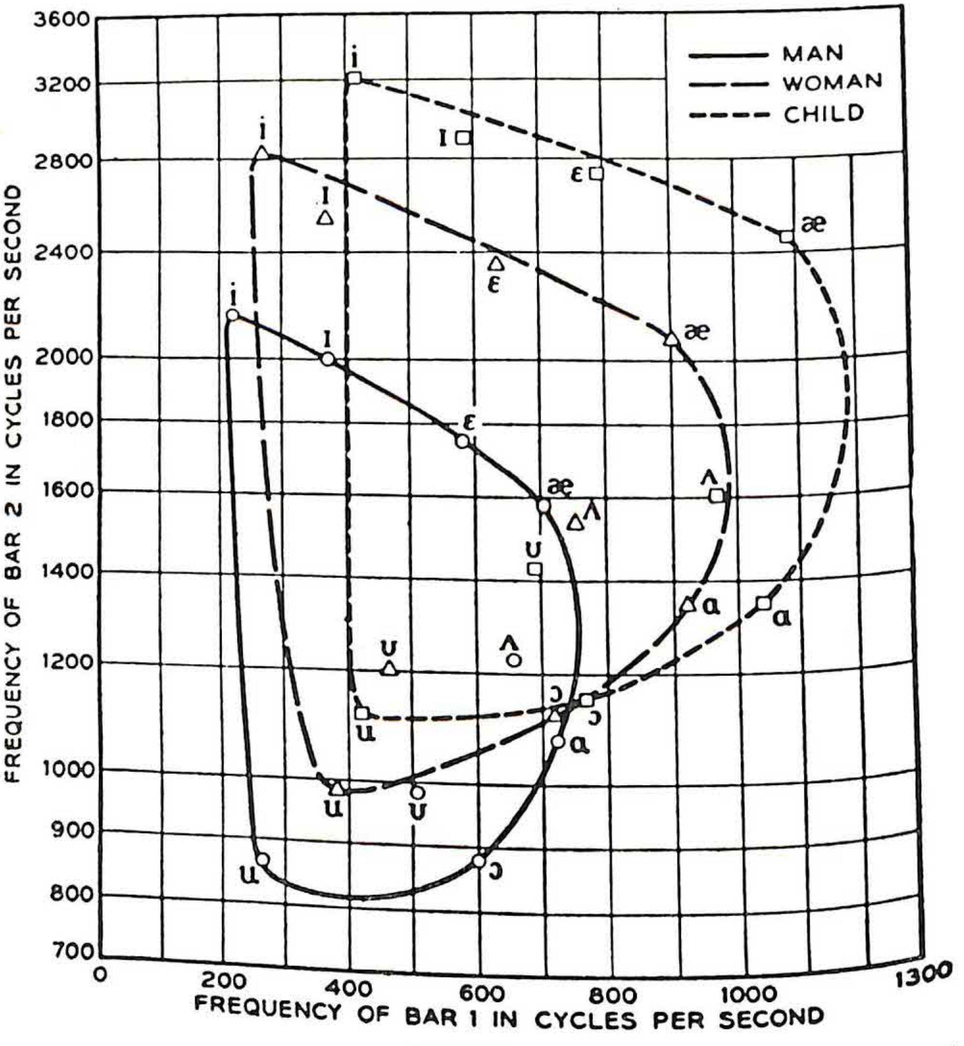

FIGURE 2

Plots . . . for the vowels as spoken by a man, a woman, and a child. (Reprinted with permission of the author, 29.)

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[3] Cooper *et al.* have devised a pattern playback machine, "which converts spectrographic pictures into sound, using either photographic copies of actual spectrograms or, alternatively, 'synthetic' patterns which are painted by hand" (5, p. 597).

In one experiment (5) bursts of noise, having constant band width but varying in frequency location, were arranged directly to precede the schematized formants of seven vowels. Listeners were asked to judge each burst of noise as one of three stop-plosives [p t k]. Curtis summarized their findings as follows:

> The most significant point is that the frequency locations for the bursts which were chosen for a particular consonant varied with the vowel which followed it. In other words the acoustic energy distribution was not an independent unit which produced a consistent consonant response (7, p. 155).

That is, the same physical characteristics of sound (in this case frequency and duration) were perceived as different phonemes by observers. It would then appear unreasonable, judging from this and other research conducted at the Haskins Laboratory, to compare, as Lynip suggests, similar spectrographic patterns that might be heard by listeners as different phonemes.

Although considerable acoustical information could be gained from spectrographic studies of infant speech, it would seem naïve and without justification to compare, without identifying phonetically, the spectrographic record of sounds uttered by adults and infants. Investigators interested in the phonetic development of speech may find spectrographic analysis a useful complement to other research techniques but not a replacement for any present means of observation.

An additional fact, ignored by Lynip, should be mentioned. In spectrographic analysis it is of prime importance that the sounds to be analyzed be recorded without interfering noises. If this precaution is not taken, speech sounds (in the spectrographic record) may not be distinguished from spurious noises. Considering that Lynip's recordings were made in the infant's home, it is not clear exactly what composite of noises, reflected sounds, and speech utterances were subjected to spectrographic analysis.

C. Experimental Investigation

1. *Collection of Data*

As an introductory attempt to determine vowel formant frequencies for infants, five children ranging in age from nine to 15 months (mean age, 11½ months) were selected as subjects. The infants, who were held by their mothers in an anechoic chamber (a room essentially free from sound reverberation), were positioned approximately 15 inches from a nondirectional microphone. The spontaneous speech utterances of each infant were recorded for approximately one-half hour.

Vowels which met the following criteria were edited from the tape: (*a*) appeared to the investigator to be "fairly good" examples of the English vowel phonemes as listed by Fairbanks (8), (*b*) did not appear to be diphthongs or triphthongs, and (*c*) were not muffled, lacking in intensity, or masked by noises made from the child's movements.

In this way 67 vowels were selected. Each vowel (or in some instances several vowels when the sounds could not easily be separated by the dubbing procedure) was dubbed onto a larger tape reel to follow the carrier phrase, "You will write." This procedure resulted in a total of 39 "short samples" of vowels preceded in each instance by the carrier phrase.

2. *Listening Session*

Ten advanced graduate students in speech pathology attending the University of Iowa transcribed the vowels in accord with the following directions:

> You are going to listen to 39 short samples of infant speech. You will be asked to transcribe the vowels you hear in the International Phonetic Alphabet, as listed in Fairbanks (8). In some of the samples consonants will appear with the vowels; please ignore the consonants and transcribe only the vowels. Do not hesitate to transcribe diphthongs, if you are sure they occur.
>
> Since infant vowels are generally abrupt and brief, each short sample will be preceded by the carrier phrase, "You will write." (Each listener was administered a sheet of paper.) You will find next to each of the numbers one, two, three, or four blanks. These blanks are for the number of vowels which will follow each carrier phrase.
>
> You will first listen to 10 short samples of infant speech. This will be a practice session; please transcribe the vowels you hear. Be sure to transcribe each vowel. If you are somewhat doubtful, make a guess as to the most suitable phoneme.
>
> You will now transcribe 10 short samples of infant vowels. Are there any questions?

After the transcription of the 10 practice samples, the first sheet of paper was taken from the judges and a second sheet was administered to them with the following directions:

> You will now transcribe 39 short samples of infant vowels. Please follow the same directions given before the practice session. Are there any questions?

3. *Results and Discussion*

Thirty-one vowels, as shown in Figure 3, which had been agreed upon by seven or more of the judges as to the actual vowel uttered were subjected to spectrographic analysis. The obtained vowel formants (the frequency of Formant 1 versus the frequency of Formant 2) are plotted in Figure 3. It can be observed from this graph that the infant vowels are, for the most part, displaced upward and to the right in relation to the vowels of adults, as reported by Kock (29). Also, some overlap among formant frequencies of different vowels and considerable variation of the formant frequencies of the same vowels may be noted. It is assumed that data analyzed from infants of a younger age than those used in this study would both extend the displacement and increase the variability. Thus, one would not, as Lynip presupposes, expect infant and adult vowels to be acoustically "comparable."

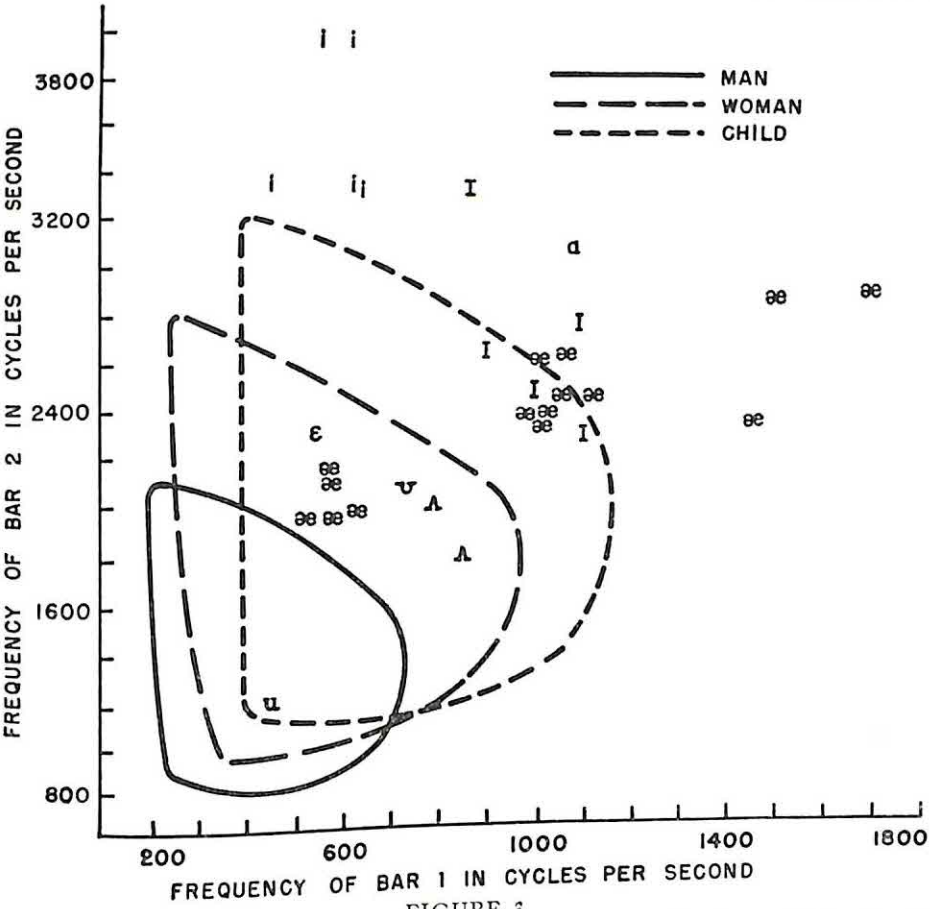

FIGURE 3

Frequency of second formant *versus* frequency of first formant for 31 infant vowels agreed upon by at least seven of 10 judges as to the actual vowel uttered. The values are shown in relation to the plots, as determined by Kock (29), in Figure 2.

D. SUMMARY

An evaluation of Lynip's thesis that adequate study of the prelinguistic utterances of infant speech must be devoid of such handicaps as the fallibilities of the human ear and of the use of phonetic systems was made. It was concluded that his assumption was illogical and unwarranted.

Thirty-one infant vowels, agreed upon by seven or more judges as to the actual vowel spoken, were subjected to spectrographic analysis. The obtained formant frequencies (frequency of Formant 1 *versus* frequency of Formant 2) were plotted in a two-dimensional graph.

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BOOKS

Now that there is a special *APA* journal completely devoted to the publication of book reviews, it is no longer necessary that other journals emphasize such publication. It has always been our conviction that book reviews are a secondary order of publication unless they carry information that is equally important as the book. However, the publication of book titles is a very important service, and we shall continue to render that service.

In any given issue of this journal, we may continue to publish one or more book reviews, but we do not consider such publication a major function of this journal. In line with this policy, we can no longer pay for such manuscripts.

(Lynd, H. M. *On Shame and the Search for Identity.* New York: Harcourt, Brace, 1958. Pp. 318.)

REVIEWED BY HENRY WINTHROP

Although written by a distinguished sociologist the volume under review is basically concerned with a significant psychological problem rather than a sociological one. In this volume Helen Lynd has devoted her efforts to an analysis and a clarification of the *sense of shame* and its relationship to the sense of self. Her treatment of the subject will possess a major appeal for certain select groups. Those behavioral scientists who are increasingly orienting their outlooks and research theories to the rôle of values in human behavior, where these are interpreted as cognitive schema, will find a number of provocative theses in this volume. Those social scientists who are beginning to demand an increase in the integrative, interdisciplinary type of study as a *fait accompli,* rather than as a program, will find a prime example of this product in the present volume. Psychologists who are making strenuous efforts to effect a *rapprochement* between academic psychology and work from various sources of the psychoanalytic tradition, will find in this volume a critical discussion of the major factors which are working against this possibility. Psychologists who are oriented to an ego-psychology and who find merit in a phenomenological analysis of the concept of self, will find several new insights furnished by Lynd along these lines. Finally scholars in the social sciences and in social philosophy, who stress the importance of historical interpretation and what Warren (9) and his co-workers and Winthrop (10) have elsewhere called *meaningful correlation,* will find these skills exercised with creative originality and abundant documentation by Lynd.

Lynd asserts that the search for self-identity has become the major psychological preoccupation of our time. Although both laymen and psychoanalysts who matured in the heyday that was Freud's, are likely to feel that the major, individual pathologies of our time have their roots in stunted psychosexual development, Lynd disagrees. She quotes Erikson (2), a psychoanalyst, who asserts that individual maladjustments are related today more to an inchoate and fragmentary sense of identity than social restraints on unhampered sexual activity or ignorance about the natural and proper expression of one's sexual life. Lynd states her thesis and the position she proposes to explore at the very beginning of her volume and the flavor of the material she proposes to wrestle with is best given in her own words.

> This search for identity, as will be apparent from all that follows, is a social as well as an individual problem. The kind of answer one gives to the question Who am I? depends in part upon how one answers the question What is this society—and this world—in which I live? . . .
>
> Above all . . . (men) *name* different aspects of (their) experiences. It is hard to find any phase of personality development or of social life that has not acquired a label with its rapidly codified weighting of association and meaning. The study of instincts may give way to the study of traits, and this, in turn, to the study of drives, motives, vectors, or variables as the terminology in favor changes; but it would seem that whatever the human experience, or aspect of personality, it could not escape being caught in the mesh of some scheme of codification.
>
> Our period of history, too, is marked by self-conscious labels. It is called a new period of failure of nerve, an age of conformity, a period of *anomie* or cultural chaos, of escape from freedom, a new age of treason, an age of longing, a decline of the West. Much is heard of the dilemma of liberalism, the distrust of the democratic process, the revolt of the masses, the failure of self-determination, the loss of command over the environment. We who live in this time are described as alienated, estranged, isolated, alone, a lost generation, depersonalized, other-directed, double men . . . (Pp. 14-15.)
>
> Elaboration of research methods in psychology and social science has kept pace with the proliferation of labeling. Observations, categories, techniques for the study of human nature were never so abundant; there were never so many people engaged in using them. If understanding of identity and of ways of realizing it could be discovered by such means, this strategic problem of today would seem assured of solution.
>
> But since every way of seeing is also a way of not seeing, it is possible that the very multiplication of categories and the very precision of techniques may sometimes act as barriers instead of as means of access to understanding. Reliance on accepted categories and methods may mean that certain phenomena essential for understanding identity escape attention. In the present climate of psychological thought any observed

> human characteristic speedily acquires a label, which encases it within one of the experimentalists' or the clinicians' categories. Extensive as these categories are, applied to some life situations they may be more constricting than informing.
>
> Certain pervasive experiences, not easily labeled, may slip through the categories altogether or, if given a location and a name, may be circumscribed in such a way that their essential character is lost. Habituation to such usage may blind us still further to the necessity of searching more deeply into the nature of these experiences . . . (P.16.)
>
> In these pages I shall attempt tentative exploration of one such experience-shame-familiar in name but far from clear in meaning. Most psychologists would agree that the cluster of phenomena roughly described as shame needs further study. There would be less agreement on the nature of these phenomena or with the suggestion that some of our current assumptions and methods in psychology and social science tend to block understanding of them.
>
> I became interested in experiences of shame through coming to recognize that concepts of guilt as they are currently used, under a variety of names, are inadequate to explain certain types of experience and certain types of personality which they are assumed to include. They and their derivatives, moreover, leave much of the sense of identity unexplained and perhaps unexplainable. It then occurred to me that further exploration of experiences of shame might help to explain some neglected aspects of personality development and lead toward greater understanding of a sense of identity. (P.17.)

Much of the present volume is devoted to the sharpening of a psychological distinction between *guilt* and *shame*. This distinction is extremely basic for Lynd's thesis. She believes that it can be shown that a sense of identity is momentarily obtained or quickened when the individual experiences a sense of shame. She furnishes a great deal of evidence from literature to indicate that sensitive writers, particularly Shakespeare, were well aware of this fact. A sense of guilt, achieved via a well-developed superego, is shown to be insufficient for self-knowledge. Guilt involves a transaction between the acting subject and either other persons in his environment or transgressions of law, public opinion, or public morality. A feeling of guilt not only does not enhance self-awareness but may actually cut the subject off from self-awareness by preoccupation with the attitudes of others, either in face-to-face situations or as introjected culture-heroes. Shame on the other hand is a transaction between the behaving subject and his ego-ideals, futurity intentions and achieved ego-extension. Guilt and shame are jointly present in some acts and separately present in others. The focus of response is on a cultural theme where guilt is involved and on a betrayal of the self-image where shame is involved.

Shame does not always have to involve a major transgression of the self-image, although it does so very frequently. Seemingly trivial occurrences may precipitate it. A sudden realization of incongruous beliefs by which we have lived may produce it, such as a sudden questioning of one's own adequacy or the appropriateness of one's values for the world of reality. Incongruity between an expectation and an outcome may bring it on. The shattering of trust in other persons and the sudden recognition that an assumed mutuality in personal relations was never really present in the first place, will produce a sense of shame over one's judgment. Shame, however, *is* more frequently associated with a major betrayal of one's self-imposed ideals. When it occurs it involves the whole self and thus succeeds in throwing a flooding light on what and who we are and what the world we live in is really like. Because of this over-all involvement, Lynd believes that an experience of shame can be altered or transcended only if some change occurs in the whole self. In this sense *conversion* or *grace* probably spring from an intense sense of shame rather than from a deep sense of guilt, contrary to traditional belief. Lynd speaks of a phenomenological guilt axis and shame axis and in order to bring out the differences between these two states, she provides a table of the psychological considerations appropriate to each (Table 1) which is produced at this point.

The basic task which Lynd has set for herself is to show the bearing of various theories in the behavioral and social sciences on studies of the sense of shame and the search for identity. Inasmuch as she is strongly convinced that the greatest portion of *distinctly human behavior* is mediated by values and meanings often of a subtle and pervasive kind, she rejects as *insufficient* those psychological theories which are in the ascendant today. She takes this stand without denying that a large part (though not the most important, in her eyes) of the behavioral repertoire of human beings, and perhaps most of the behavioral repertoire of infra-human organisms, may be accountable for by the theories which are dominant today. She directs her critical shafts against several prevalent theories which, by ignoring or limiting the rôle of value and symbolism in human behavior, may miss such pervasive experiences as shame, and which tend, by implication, to limit the possible variety and richness in identity, which is available to man.

Her first target is what she calls the libido-fund theory of Freud, which, she asserts, is analogous to the wage-fund theory of economics. This is Freud's theory of the limited amount of psychic energy available to each human being. This limited energy must be deployed to one purpose or another but can never be enhanced or enriched. For this reason she refers to it as a theory of

TABLE 1

Guilt axis	Shame axis
Concerned with each separate, discrete act	Concerned with the over-all self
Involves transgression of a specific code, violation of a specific taboo	Involves falling short, failure to reach an ideal
Involves an additive process; advance to healthy personality by deleting wrong acts and substituting right ones for them	Involves a total response that includes insight, something more than can be reached by addition
Involves competition, measurement on a scale, performing the acts prescribed as desirable	Involves acting in terms of the pervasive qualitative demands of oneself, more rigorous than external codes; each act partakes of the quality of the whole
Exposure of a specific misdemeanor, with emphasis on to whom exposed; exposure of something that should be hidden in a closet	Exposure of the quick of the self, most of all to oneself; exposure of something that can never be hidden in a closet, is in the depths of the earth or in the open sunlight
Concern about violation of social codes of cleanliness, politeness, and so on	Concern about unalterable features of one's body, way of moving, clumsiness, and so on
Feeling of wrongdoing for a specific harmful act toward someone one loves	Feeling that one may have loved the wrong person, or may be inadequate for the person one loves
Being a good, loyal friend, husband, wife, parent	Having an overflowing feeling for friend, husband, wife, children which makes goodness and loyalty a part of the whole experience with no need for separate emphasis
Trust built on the conception of no betrayal, no disloyal act, as a preliminary to giving affection	Trust that is a process of discovery which gradually eliminates fear of exposure, which is not the result of an act of will but unfolds with the unfolding experience
Feelings of anger, jealousy, meanness for certain socially recognized causes	Inwardly deep feelings of anger, jealousy, meanness in outwardly slight situations known to oneself only
Emphasis on decision-making; any decision is better than none	Ability to live with some indecisiveness (multiple possibilities) even though it means living with some anxiety
Feeling of guilt toward someone who has denounced one for adequate or inadequate cause	Feeling of shame toward someone who trusts one if one is not meeting that trust
Emphasis on content of experience in work, leisure, personal relations	Emphasis on quality of experience, not only on content
Surmounting of guilt leads to righteousness	Transcending of shame may lead to sense of identity, freedom

psychological and moral scarcity. As a result of this limited deployment, object-cathexes in human life are also limited. This is because *displacements,* themselves, arise only in relation to a limited number of primary *object-choices* and the scarcity of types of object-choice is, itself, due to the few instincts available to the human organism. Thus human choice tends to be impoverished. Such impoverishment implies a restriction on the development of personality and a weakening of the sense of self through a limitation of the contexts in which a sense of shame may develop. It leaves little scope for the possibility of patterns of experimentation with individual and social values that are very much different from those prevalent today. In effect this theory comes in for Lynd's criticism precisely because it might have justified the caption, to paraphrase Karl Popper, "the open individual and his theoretical enemies."

Two other theories come in for criticism. The first of these the author calls the compensatory theory of personality development, stemming from Freud and found to some extent among contemporary, experimental learning-school theorists. By the compensatory theory Lynd refers to the doctrine that much of behavior, culture, learning, and art is *nothing but compensation or overcompensation* for biological drives thwarted by the culture. This gives rise to such teachings as those which assert that sympathy, friendship, and trust develop out of the enfeebling of the sex drive, that social feelings towards liked persons must culminate in aversion and hostility, which are repressed, or that the desire for knowledge is a form of the possessive instinct. A third class of theories whose shortcomings are analyzed by Lynd includes theories of the stimulus-response, pleasure-pain, and reward-punishment variety. As Lynd evaluates theories of this type, they all assume motivation is strictly hedonistic in nature, that is, according to these theories, individuals are always driven to seek pleasure and to avoid pain and in interpersonal relations other human beings exist only as objects which furnish us with or deprive us of desired pleasure. This restrictive interpretation of human motivation, implied by hedonistic theories, is rejected by Lynd in the following words.

> . . . Reward-punishment theories built on a model of psychological and moral scarcity make impossible the understanding of some of the deepest personal experiences, such as shame, identity, independent thought, and perception of and sense of moral values beyond those of a given culture. The inadequacy of such a model for human understanding is coming increasingly to be recognized (P.105).

Lynd devotes a good portion of the volume under review to a discussion of the limitations of contemporary thinking in psychology and the social sciences. One of these limitations is the failure to take historical change seriously. There are *at least two* considerations involved in this criticism. One is that the synchronic approach in the behavioral and social sciences misses the fact that much behavior is to be *understood only* in terms of maturational and developmental considerations. The other is of more direct professional concern, namely, that rigidity in behavioral theory can be somewhat lessened if psychologists were somewhat aware of the importance of the sociology of knowledge in indicating the historical, accidental roots of some of their theoretical convictions and concepts. Many concepts would not be seen as universal and timeless explanations of certain aspects of behavior, if the degree to which the Zeitgeist has given them birth, were better understood. Thus, for example, the psychiatric cliché of the rejecting mother cannot be understood except as part of the development of the conditions of American pioneer life and the special characteristics of the American Puritan tradition.

Lynd rejects in psychology what has elsewhere been called the *paralysis of analysis.* The historical tendency to think atomistically about behavior, to study well-controlled and isolated fragments of it and then try, or hope to try, to assemble all the findings in order to present a complete account of the behavior of the total organism, is seen as futile by Lynd. This atomistic bias which she rejects has been felt for a long time to supply greater objectivity in scientific research. Lynd's own, holistic outlook leans upon organismic, Gestalt, and field theory. She states the familiar point of view that the properties of laboratory demarcated behavior, as these are isolated from total context, may be different from the manner in which the individual functions in the total, cultural field. In order to emphasize what is involved in *understanding* human behavior and to avoid what she takes to be unwarranted simplifications of the nomothetic bias, Lynd reminds us that a subject's perceptions are not detached, but are part of his whole personality. For these reasons she asserts that there is no such thing as objectivity into which the thoughts, feelings, and values of the perceiving self do not enter. The observer enters into his observations in one way or another. Failure to be aware of this fusion results in failure to separate self from other.

Lynd also criticizes what she takes to be the limited understanding of the rôle of language in contributing to a sense of self-identity. Logical rigor, analysis, and the current, ascendant emphasis on signs or signals, she feels, may limit the kinds of experience to which we give attention. By impoverishing the *variety* of experience to which we may give attention, we reduce the variety of self-identities which individuals can create for themselves. Many of the psychological states so important for the development of identity are, in Lynd's estimation, impervious to capture or comprehension in a language of analysis. In the realms of phenomenal sensitivity, value-disposition, and value-enactment, a language whose terms possess surplus meaning (which would be anathema for strictly analytic purposes) may, according to Lynd, actually be more desirable rather than less desirable. Her own way of putting it drives home her point more adequately.

> . . . A language that is confined to labeling rather than defining, to denotation at the expense of connotation, does not have the means of expressing experiences whose nature includes ambiguity and surplus meaning. It omits from its purview some of the most significant dimensions of human nature, and possibly distorts others that it may seem to express more fully.
>
> Limited exactness and the elimination of ambiguity in the use of language express the reductionist, nothing-but tendencies that run through the aspects of contemporary study discussed . . . Necessary as such use of language is for certain purposes, it does not allow verbal expression of pervasive experiences such as shame and the sense of identity, and thus tends to constrict understanding of them (P.121).

In order to illustrate the evocative and expressive possibilities of non-analytic language in enhancing awareness of self, Lynd deals with a number of considerations along these lines. She discusses the Sapir-Whorf hypothesis about the nature of perception, the work of Fromm-Reichmann (3) and Sechehaye (7) in communicating with schizophrenics, the artistic experiments with expressive language in the work of Virginia Woolf, D. H. Lawrence, James Joyce, Gide, and Proust, and the rôle of ambiguity and surplus meaning in language, as revealed by Empson (1). She also spends some time discussing the loss of the diffused quality of early childhood experiences, as these are reported by certain psychiatrists. During the years in which adult, verbal sophistication appears, these experiences are *unrecallable* because they were once inappropriate to the crude language development in children. Lynd furnishes three reasons why expressive language may help to recapture experiences not available to analytic language. Absence of expressive language may lead to atrophy and lack of awareness of idiosyncratically charged, phenomenal states. Ranges of mutual exploration may be cut off and unnecessary misunderstanding arise, if one does not try to enrich the communication of subtle, psychological experience which is only crudely apprehended by nonverbal communication. Creation of expressive language symbols can bring unconscious creative forces into relation with conscious effort and can give form to material otherwise relatively inaccessible to self and attention.

Lynd reserves her heaviest criticism for the assumptions made by behavioral scientists, particularly psychotechnicians, that rôle-playing and adjustment contribute to a sense of identity. She rejects the notion that the self is nothing but the aggregate of the rôles it voluntarily assumes or is forced to play. The fulfillment of social rôles is enacted on behalf of the guilt axis while the development of a sense of self is dependent upon a shame axis. The individual in the latter case must adhere to self-prescribed rôles, that is, behavior in conformity with self-ideals. It is true that some social rôles are both necessary and desirable but the desire to fulfill them must be self-imposed rather than socially imposed and the desiderata which such social rôles fulfill must be understood and acquiesced in by the individual. Self-identity is rarely achieved in our society because individuals fail to distinguish what can and cannot be accomplished separately via guilt or shame. Lynd puts it as follows.

> . . . In our society a person is inclined to be more specifically conscious of behavior on the guilt axis, of transgressions that are culturally defined and redeemable, and of their counterparts that add up role by role to a righteous, mature, adjusted life. He can perhaps at relatively small emotional expense fulfill the requirements society makes of him. But identity built on the guilt axis may be less enduring, more likely to break down into its parts. If its loyalties are based mainly on social requirements, the sense of self may change if these requirements shift.
>
> Identity based more on the shame axis may go deeper and be more of a continuous process of creation with less easily dissipated *Gestalts* . . . In a society more directly and variously expressive of human desires the guilt axis and the shame axis, role fulfillment and personal fulfillment, might more nearly coincide (p.231).

Depersonalization, through the adoption of socially approved mechanisms to protect the self from psychological exposure, is achieved through social poses. If these become permanent they replace the unfinished self and identity is difficult to recover. In the 1920's, says Lynd, the popular pose was to combine indifference and success and to take nothing seriously. In the 1950's depersonalization appears in the form of rôle-playing. If one rejects the rôles prescribed by one's immediate culture in favor of more individual or more widely human values, one tends to be alone. When persons are expected to meet and take each other in a variety of rôles that represent what they are supposed to be by virtue of their place in society, this may act to block the discovery of who they are as individual human beings.

Openness to other personalities helps to create a sense of self-identity. However, where such openness is abused and shame created for an individual by exploiting the psychological content of such openness, individuals prefer to hide behind their assumed rôles. When such hiding becomes a habit then, indeed, self-identity is never achieved, and selfhood will be seen by the observer as an aggregate of others' rôles. As a result the perspective on values becomes limited and the spectrum of values offered by the culture is more limited still. A tendency arises to see the current spectrum of offerings as indicative of human possibilities rather than as a cultural and historical accident. Adjustment is then defined as conformity to this spectrum and awareness of the realm of the value-possible dies out. With it dies the feeling that social experimentation is possible and that the individual can, himself, contribute to such experimentation. In short the relationship between freedom and authority on the one hand and its bearing upon the ability to achieve self-identity, on the other, has to be continually restated and remolded in time. This must be done to achieve a sense of shame, so that the possibility exists of falling short of self-ideals unrelated to prevailing social values, ideas, and rôle-expectancies.

Lynd makes the point that individual and social pathology are often alleged to be the result of *hubris* or pride which is often contrasted with a sense of shame. But she points out that there are two kinds of pride. There is pride that is arrogance and satisfaction in power and superiority over others and which arises from a sense of oneself as contemptible, ridiculous, or dishonorable. It depends on *external* approval. Then there is the pride which involves honor, inviolability, and freedom through selective ego-extension. For the Greeks this is *philotimo* or self-respect. If one has philotimo then hubris is unnecessary. A sense of identity and an unneeded and decreasing sense of shame is furthered by philotimo. Hubris in modern man provides the opposite effects. Lynd feels that the human condition can be remade by philotimo and by our willingness to experiment with the enlargement of value. Psychological theories which suggest that such behavior is alien to the human personality, increase the difficulty of achieving identity. Social theories which overstress adjustment to prevailing values not only do the realm of historical possibility an injustice but also work towards the formation of a closed society and increased alienation.

Lynd's volume provides a much needed balance against the prevailing theoretical monisms in the behavioral and social sciences. It is unnecessary to agree with her in order to appreciate her integrative approach. The book's merits lie in emphasizing the rôle of values in understanding the human condition, in reminding the psychologist of the importance of the time dimension in human development, and in admonishing him on the degree to which their efforts are servicing conformity in our time. Most important of all, it emphasizes that there are realms of experience which may be refractory to the positivist tradition and approach, whose neglect may tend to produce limited understanding of behavior, out-of-focus interpretation of it, and premature restriction on expectancies in behavior available to man and society. The only major fault of the volume, which must be mentioned here, is that it does not do justice to the existentialist stress on the inevitable isolation of man, the importance of death in providing perspective on life, and the fact that the greater man's measure of identity the greater must be the measure of his homelessness in some sense. Lynd is somewhat critical of these existentialist emphases apparently because she believes that they may easily pass over into some kind of a moral nihilism. At the same time they may lead to a loss of the euphoria which directing and integrating values are presumed to furnish. Finally they may cut off that perspective which leads one to recognize that the alienative pathology of a culture which has lost its vital center, may be chiefly the result of a crisis in social transition, and that this too may pass away. She states her fears in the following words.

> The existentialist stress on the inevitable isolation and homelessness of man, and on death as the most important fact in life, tends to minimize, not only positive affirmations of joy and courage, but the processes of history and the varying possibilities in different concrete historical situations . . . It is not necessary to be blind to the limits that surround life to believe that changes in the social situation can bring about some changes in the realization of human possibilities, and that within the necessities significance may be found. History continues to be an attempt by man to make a home for himself in the world, and philosophy a heightened interest in life, not a trained indifference to life (Pp.251-2).

I believe this interpretation of the existentialist position to be somewhat mistaken and somewhat strained. The melancholia of a Kierkegaard or the pessimism of a Nietzsche may justify some of her doubts. On the other hand Berdyaev's psychological affirmations, religious concerns, and political struggles, are a living testimony to the fact that an existentialist bias need not have the outcome which Lynd fears. She, herself, recognizes clearly enough that a religious existentialist like Tillich (8) is in the forefront of the battle to recover moral self-identity. Likewise there are several psychologists and psychiatrists, operating with an existentialist bias in psychotherapy, who seem anxious to stress the healthful aspects of affirmation and of historical and social consciousness. This may be seen from May's (5) own work and that of various European practitioners with an existentialist background, who contribute to a well-known volume edited by May (6) and his coworkers. Heinemann (4) has, I think, demonstrated clearly enough that existentialism, as a *regulative idea,* will help to deal with the psychological problem Lynd has taken so seriously.

Lynd's concern over the possible undesirable effects which existentialist philosophy might conceivably produce in trying to achieve a sense of self-identity, is, at the worst, a very minor flaw. Her own theses represent a much needed amalgamation of psychological viewpoints with those of the social sciences. She has furnished a rather penetrating analysis of the importance of a sense of shame in developing a sense of identity, thereby adding to our phenomenological understanding of the growing literature on selfhood. Though outside the main stream of academic emphasis and approach, her book can still serve to enlarge the interest and understanding of the concept of selfhood for psychologists of various clinical and centrist persuasions.

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STUDIES ON THE BASIC FACTORS IN ANIMAL FIGHTING: I. GENERAL ANALYSIS OF FIGHTING BEHAVIOR*

% The Smithsonian Institution

ZING YANG KUO

A. INTRODUCTION

These studies were initiated in 1927 in Shanghai. The bulk of the work reported in this series was done in Shanghai between 1927 and 1929 and in Hangchow from 1929 to 1936. Most of the experimental routine was carried out by two assistants and four technicians.[1] From 1936 on, social and political upheavals, the Japanese invasion and consequently World War II, and finally the Communist revolution had made continuation of the investigation impossible. However, in recent years the work has been resumed in small measure in the writers' own residence and additional data have been gathered for these reports and several experiments on fighting are still under way.

Working in a locality where adequate scientific literature is not available, the writer wishes to ask for great indulgence if he fails in each article of this series to give the usual historical account of previous publications on the related subjects, and to cite references which have direct bearings in discussing results of each experiment. The writer wishes also to take this opportunity to express his gratitude to several American scientists who have kindly sent him reprints of their work which have helped to reduce the extent of the writer's ignorance of the existing literature on the subject, and especially to the late Dr. Edward Chase Tolman and to Dr. Leonard Carmichael both of whom have made kind effort to help him secure references and whose constant persuasion to publish these reports are to a considerable extent responsible for the writer's final decision to do so.

Since 1927 when the writer began these investigations, the literature on animal fighting or aggression, domination, and subordination has been quite extensive. Incomplete knowledge of the literature has made it impossible for the present writer to evaluate the work of the investigators in previous publications. However, after carefully reading the available papers the writer seems to feel that the following points have not received sufficient emphasis by the investigators in the previous communications.

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* Accepted for publication by Leonard Carmichael of the Editorial Board; received in the Editorial Office on February 10, 1960, and given immediate publication as a public service. Perhaps younger psychologists might need to be told that this scientist's important publications can be found in the old *Psychological Index*.

[1] For important reasons their names cannot be revealed with the exception of B. T. Liang whose paper on "The effect of gonadectomy and tescular transplantation on habit formation and retention in the white rat" was published in the *Journal of Genetic Psychology* in 1933, Vol. 43.

(*a*) Fighting is a very complex behavior pattern which requires detailed analysis and grouping so that each animal under investigation could be scored or classified into a number of types and the same scoring or classification must be used in different experiments and in different species of animals so as to make the fighting tendency and fighting capacity in different species comparable. The mere statement that a given animal under study is more or less pugnacious or that the fighting tendency is increased or decreased as it is most commonly made in the previous communications is rather vague and lacks the scientific specificity and quantitative element in these studies. (*b*) Except those studies dealing with chickens and white mice, the animals employed in the previous reports were so few that any conclusions drawn from the results were open to question as to their statistical reliability. (*c*) The factors affecting fighting are so many and so variable that unless these factors are systematically kept under constant conditions while one single factor is subjected to experimental variations it would be extremely difficult to interpret or evaluate the results of a given experiment. Thoroughness and systematic control and variations of the various factors in fighting have not been carefully done in most previous reports. (*d*) There seemed to be a general lack of standard experimental procedure which could be applied to different types of experiments and to experiments on different species.

The lack of emphasis on these four points is not a criticism of the previous publications. It serves merely to point out the fact that while observations on fighting reported by naturalists would inevitably contain certain elements of casualness, other reports on fighting were made by investigators whose primary interest lies either in some physiological effects (such as the effects of hormones) or in aggressions of the experimental animals in group life or in social hierarchy of animal society, rather than in 'fighting' *per se*. In other words, "fighting" in these investigations which themselves are important and valuable contributions to the scientific understanding of animal behavior was merely one of a number of issues revolved around the central themes which were either aggressiveness in interindividual relationship or dominance and submissiveness and social ranks in animal social life. On the other hand, in our own studies, "fighting" is the central theme, and as such a strict and standardized laboratory procedure and thoroughness and systematic control and variations of the various factors affecting the behavior patterns of fighting have become our basic requirements.

B. GENERAL ANALYSIS OF FIGHTING BEHAVIOR

In the following reports a large number of species were employed. They are fighting crickets, Siamese fighting fish, Japanese grey quails, chickens, and dogs and in the case of experiments on the effects of group life on fighting behavior, a large number of species of fish and more than 30 species of birds, as well as cats, rats, rabbits, and guinea pigs were employed. Our experimental records show that there were varieties of behavior patterns related to fighting in these species. To facilitate comparison of the behavior patterns in different species as well as comparison of the effects of various factors on fighting behavior, and to simplify the presentation of the results of the various experiments to be reported in this series we have assembled and classified the behavior patterns of fighting into 23 types shown in Table 1. This table will serve as a standard reference throughout the subsequent articles of this series. In the subsequent articles with certain exceptions, the behavior patterns of each animal employed in the experiments will not be described but will be referred to the type numbers in this table.

In the previous publications which are accessible to this writer, the words "Fighting" and "Aggression" are sometimes used interchangeably. This is rather confusing. An aggressive act of an animal may or may not involve a physical attack on another animal. And even when it does, the "attacking" action alone does not necessarily constitute "fighting." It is only when an animal attacks another animal, which in turn bites back or pecks back as the case may be, or both animals make attacks upon each other simultaneously that "fighting" actually takes place. It takes more than one animal to start a fight. Therefore, throughout our reports, the word "aggression" will not be used and only when an attack of one animal is made upon another which replies in kind we shall call it "fighting." On the other hand, when an attack does not provoke a counter attack, the word "attack" alone or the expression "attack without counter attack" instead of the word, "fighting" will be used. In this connection it is to be emphasized again that in our investigations the central theme is "fighting" *per se*. All the other aspects of behavior such as *dominance and submission* which have formed the main theme in many investigations in previous publications will be noted and occasionally mentioned in our reports only when they are directly related to "fighting." It is further to be pointed out that in our studies little attention was given to social orders or "rankings" in animal social life which itself is a very interesting aspect of animal society and which has also been extensively studied by a number of investigators, but which was rather beyond the scope of our main theme. Since our chief interest was in "fighting" of the individual animal,

TABLE 1

Type number	Description of behavior pattern of fighting
I.	(*a*) Attack on any object in sight regardless of sex, species, even dummy objects, or animals of much bigger size than itself; (*b*) fighting once started will not stop even at point of exhaustion, or serious injury; (*c*) will fight both inside and outside fighting pen; (*d*) after recovery from exhaustion or injury, will, even if previously defeated, rush to challenge its old enemy; (*e*) will not stop attacking even if its opponent stops fighting, runs, or surrenders.
II.	Same as Type I but without (*e*).
III.	Same as Type II but will not fight its old enemy once having been defeated by him although still ready to fight new opponents.
IIIA.	Will not fight again with any animal once it is defeated.
IV.	Same as Type III but will not attack dummy objects or animals of other species.
V.	Same as Type IV but will attack other species.
VI.	Same as Type V but will not attack animals of opposite sex of same species.
VII.	Same as Type V but will fight only inside the fighting pen.
VIII.	Same as Type VII but will fight only when attacked.
IX.	Will put up a fight but for only a short duration, and jump out of the fighting pen whether winning, or losing, or still indecisive; even though often initiates fighting without provocation.
X.	Will fight only if nesting or domain is invaded, or wherever meets old enemy, or in competition for food or female. But fighting will stop once the intruder withdraws or shows submission.
XI.	Same as Type X but will attack any stranger or intruder regardless of sex.
XII.	Never attacks other animals. Always friendly or indifferent to other animals regardless of sex, but will defend, feebly and of short duration.
XIII.	Same as Type XII but will not defend even if attacked, will either run, or show submission.
XIV.	Same as Type XII but will attack strangers.
XV.	Indifferent to other species, but friendly to own species, even strangers.
XVI.	Friendly to other species with whom it was brought up, but hostile, or indifferent to, or will attack animals of same species which are strangers to it.
XVII.	(*a*) Living peacefully in same quarters with own and other species with great varieties of size; play with each other, grooming each other, roosting closely together at night, sitting under the wings of bigger birds. (*b*) In case of fish, swim peacefully, although at times, one chases another (same species), or attempts to bite another, or both display fighting attitude, and occasionally engage in a brief combat for one or two seconds (always same species), very occasionally one chases another of different species. (*c*) Engaged in a brief fight in the midst of playing (dogs), or in competition for food, hissing (cats), pecking (birds), biting (rats), or when roosting at night pecking one another (birds).
XVIII.	Same as Type XVII but without (*b*) or (*c*).
XIX.	In interspecies living, the behavior of different parrots, Chinese blue birds, canaries, and sparrows belongs to special categories, each of its own. This will be described in the fifth article of this series.
XX.	Attempt to attack, but attacking is immediately stopped when the would-be attacked animal shows friendly or submissive attitude.

TABLE 1 (*continued*)

Type number	Description of behavior pattern of fighting
XXI.	Attempt to attack at first, but attitude immediately changes to friendly, or submissive, or even turns tail, when the would-be-attacked animal stands firm, or shows readiness for a fight.
XXII.	Attempt to attack any animal of same or different species, regardless of size or sex whenever the two animals come face to face. When one runs, the other chases; chase stops, when runner is at some distance. Attack or attempt at attack is often aborted, when the opponent stands firm. Chase is stopped also when the chased animal stops running and turns around to face chaser.
XXIII.	In group fighting, in dogs the behavior patterns belong to a special category which will be described in the sixth article of this series.

we had occasions to study animal life in a group in so far as group life affects the behavior patterns of fighting in the individual animal.

It was our considered opinion that since our aim was to observe two animals in combat, it would not be scientifically justified to interrupt the fighting in order to save the animals from severe injury or from death. In these studies which involve several hundreds of animals, in no case did we ever attempt to interrupt a combat before it reached a definite decision (except during training).

It is to be specially noted that throughout these series of experimental reports, one thing is particularly and pointedly omitted, that is, no attempt will be made to apply the results of animal experiments to human social life as the writer considers such an attempt scientifically very unsound indeed, tempting as it is. As has been pointed out, the behavior patterns of animal fighting are very complex and variable, it may be even not justifiable to apply the results of experiments on one species to another species. It will be very far fetched indeed to apply the results of experiments on fighting crickets, or Siamese fighting fish, or Japanese grey quails to human warfare, to psychiatry, or even to international conflict. One has only to be reminded that interpersonal relations, not to say international relations, are so complex and so complicated and so variable that it would be quite a big leap from scientific ground to interpret human fighting in terms of animal fighting. This is not to say that the results of animal studies are not applicable to human life. Far from it! Our point is that when such an application is attempted great caution must be taken against oversimplification of human behavior in order to fit in with the results of animal experimentations and against the great danger of anthropomorphismic implications as well as the equivocalities of terms in human social relationship (the word "fighting" has been used even in recent psychological writings with many different meanings which hardly have anything to do with physical combat of animals), and especially the danger of employing psychoanalytical concepts and subjectivistic terms of the pre-Watsonian era as several recent publications on animal fighting or aggression have already done.

% Dr. Leonard Carmichael
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The Journal of Genetic Psychology, 1960, **96**, 207-216.

STUDIES ON THE BASIC FACTORS IN ANIMAL FIGHTING: II. NUTRITIONAL FACTORS AFFECTING FIGHTING BEHAVIOR IN QUAILS[*]

% The Smithsonian Institution

ZING YANG KUO

A. PROBLEM

In this and the following two reports (Studies III and IV) the subjects used for experiments on fighting were Japanese Grey Quails *(coturnix coturnix japonica)*. These birds are now rather commonly raised in China, especially South China, since they were brought from Japan many decades ago. They are small in size, weighing only from about three to five ounces. Their food habits and behavior are quite similar to those of domestic fowls and are used in certain parts of China as food and their eggs are consumed as chickens' eggs. When trained, they become excellent fighters. They fight with great ferocity and once fighting is started, it may last for many minutes. In some parts of South China, grey quails are employed in fighting and for gambling purposes. On account of its size and its docility towards the handler, its food habits, its comparatively lower cost in breeding and feeding than chickens, especially its propencity to fighting, it has become the animal of choice for our studies on fighting behavior although we have also for comparative studies employed a large number of dogs, chickens, Siamese fighting fish, crickets, and a number of other species in this series of investigation.

When we first started these investigations we planned to use other vitamins besides vitamin B_1 as well as protein as potent factors in animal fighting. But at the time this part of the research was being carried out thiamin was the only vitamin available in pure form. Therefore, in our report in this article our experiments are limited to two nutritional factors, namely protein and thiamin.

B. GENERAL METHODS

1. *Isolation, Training, and Test Fights*

There are three experiments in this report. All the birds used in these experiments were kept separately in isolation soon after they were hatched and went through the same training schedule as described in the fourth article of this series until they were 10 months old. From the eleventh month on each animal was given a test fight which is also described in the fourth article. During the training period fighting was confined to the fighting pen, but in the test fights, fighting outside the fighting pen was also tested. The test fights were given at a three-day interval (in case a bird was seriously wounded in fighting, test fight was suspended until the wound was healed) each bird receiving five tests. The behavior pattern of fighting in each animal as observed during the test fights were classified according to the types numbered and described in Table 1 in the first article of this series. Then, variation from the standard diet was commenced according to the purpose of each experiment. After the tests were completed standard diet was restored and if further observation was required, one week after, another five test fights were given to the bird at the same interval. Otherwise, the birds were returned to their individual cages as reserves for further experiments. In each test fight each bird was matched by another bird belonging to the same type-number of fighting patterns, so as to assume relative equality or matchability of two birds in combat. When the test fighter was upgraded or downgraded in type number of fighting pattern, in its next test, its opponent would also be changed accordingly. We had in reserve a large number of quails both male and female, ranging from 10 months to nearly two years old, each living in a separated cage with its type-number of fighting patterns tagged to the cage, trained in fighting with the standard procedure, given the same standard fighting tests, and living on the standard diet throughout. These reserved animals were used from time to time for fighting matches with experimental animals and as controls. The reserved fighters, when not in use, were given two practice fights each week so as to preserve the fighting habit. (By practice fighting we mean fighting without being allowed to reach a decision.)

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2. *Standard Diet*

The standard diet for quails in our investigation consists of one level tablespoonful of dry brown rice (unpolished) mixed with a small quantity of salt and powders of charcoal, calcium, and iron, and three drops of cod liver oil and half teaspoonful of orange juice. Greens and wheat germ were used and orange juice omitted when the animals were not used for experiments on nutritional effects. This was the daily ration for each bird. The food was given to the bird at a fixed hour of the day and the bird must finish its eating in 10 minutes after which the food dish was withdrawn. Water was placed in the living cage to which the bird had free access at any time.

3. *Standard Weight*

We were informed by the gamblers in quail fighting that the body weight of the quail effected the qualities and capacities of fighting. Our investigations have confirmed this observation (see Table 1). From Table 1 it appears quite clear that few quails which were far below 100 grams or far above 130 grams could be trained to be good fighters (Types I to VIII, except Type IIIA). We, therefore, chose the weight range from 100 grams to 130 grams to be our standard weight for a good fighter in quail. When an animal began to show sign of increasing weight beyond the maximum weight, the daily ration of brown rice would be gradually reduced until its weight is brought down to the standard. An animal which had a tendency to decrease weight, would be encouraged to eat more brown rice until the minimal standard weight was reached. Every day each bird was taken from its living cage and placed in a large room to run or walk about in the room for 30 minutes.

C. THE EXPERIMENTS

1. *Experiment I*

It must be noted that although 23 types of behavior in fighting were listed (see Table 1, Article I) as far as quails were concerned which were brought up in isolation and were trained with the standard procedure, only the first eight types and Types IX, XII, and XIII were observed. Other types of fighting patterns were observed only in birds which were not brought up in strict isolation and were not subjected to the rigid course of training (see the fourth article). We considered Types I to VIII (except Type IIIA) as good fighters, Type IX, XII, and XIII as poor fighters.

In Experiment I 30 poor male fighters were used as experimental subjects and another 30 as controls. In this experiment as well as in other experiments with poor fighters, the birds were re-trained according to the standard training procedure and then given five test fights to determine the effect of thiamin. In this experiment each experimental animal was given daily 1 mg. of thiamin powder in addition to the standard diet. The control animal received no additional food besides the standard diet. One week after the daily administration of thiamin both the control and experimental animals began to receive re-training as stated and then were given five test fights. After the test fights, the food again was restored to the standard diet, and one week after another five test fights were given with the same three-day interval.

The results of Experiment I are summarized in Table 2. (In this as well as in the following tables the fighting type numbers are based on the records

TABLE 1

ALL 10-MONTHS-OLD MALES

Fighting type Weight in grams	I No.	I P.C.	II No.	II P.C.	III No.	III P.C.	VI No.	VI P.C.	V No.	V P.C.	IV No.	IV P.C.	VII No.	VII P.C.	VIII No.	VIII P.C.	Total	P.C.
70																		
80					2	2.30			1	1.27							3	0.50
90	4	5.19	2	2.53			1		1	1.27			2	2.79	5	5.81	15	2.49
100	15	19.28	15	18.99	14	16.09	18		10	12.65	13	21.66	8	11.16	18	20.93	111	18.24
110	20	25.97	22	27.87	20	23.18	21		22	25.30	14	23.34	20	27.89	22	25.59	161	26.70
120	23	29.90	26	32.91	30	34.49	12		20	25.30	20	33.34	20	27.89	21	24.42	172	28.52
130	12	15.59	12	15.14	20	23.00	10		20	25.30	13	21.66	15	21.12	16	18.61	118	19.57
140	2	2.60			1	1.15			4	5.06			3	5.11			10	1.66
150	1	1.30	2	2.53									1	2.38	4	4.65	8	1.50
160							2		1	1.27			2	2.79			5	0.83
Total	77		79		87		64		79		60		71		86		603	

TABLE 2

Type number of fighting pattern	Before administration of thiamin				After administration of thiamin				After restoration to standard diet			
	Experimentals		Controls		Experimentals		Controls		Experimentals		Controls	
	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.
I					1	3.33						
II					1	3.33	1	3.33			1	3.33
III					4	13.33			2	6.67		
IV					3	10.00	2	6.67	3	10.00	2	6.67
V					3	10.00			2	6.67		
VI					2	6.67			1	3.33		
VII					2	6.67			1	3.33		
VIII					4	13.33			4	13.33		
IX	10	33.33	10	33.33	4	13.36	9	30.00	7	23.33	9	30.00
XII	10	33.33	10	33.33	4	13.36	8	26.67	5	16.67	8	26.69
XIII	10	33.33	10	33.33	2	6.67	10	33.33	5	16.67	10	33.33
Total	30		30		30		30		30		30	

of the last test fights.) It will be seen from the table that two-thirds of the experimental animals are upgraded in their fighting capacities, that is, change from poor to good fighters, whereas only 10 per cent of the controls were upgraded. In our experience with many hundreds of quails, it is normal that without the influence of experimental factors such as nutrition, hormone and environmental factors by retraining or by repetition of test fights, etc., from 5 per cent to 10 per cent of the animals will be either upgraded or downgraded. In our records these effects by thiamin in many birds became visible two or three days after B_1 was added. It is interesting to note also that after the withdrawal of the additional vitamin and restoration to the standard diet, only 7 of the 20 birds were downgraded, that is, return to become poor fighters again. In other words, 43.3 per cent of the total 30 birds still remain good fighters 20 to 22 days after the withdrawal of the additional thiamin while there is no significant change in the control group. There are two possible explanations for this phenomenon. (*a*) The blood level of thiamin may remain still high even some three weeks after the additional B_1 was stopped. And (*b*) habituation of fighting behavior may work in both ways: (*a*) many birds remain poor fighters despite additional thiamin, and (*b*) others would remain good fighters once they were promoted from the poor fighters group by additional thiamin even though the promoting substance had long been suspended. While the problem of blood level in this connection remains to be solved in a bio-chemical laboratory, it must be pointed out here that throughout our investigation on fighting behavior in animals, habituation has been found to be a very potent factor in determining the behavior pattern of an animal. We shall take up this matter in some detail in the fourth article of this series.

2. *Experiment II*

In this experiment 60 good fighters (males) were selected, half of which were used as experimental animals, the other half as controls. Every thing in the experimental procedure was exactly the same as in Experiment I, except that, instead of the additional thiamin, white rice was substituted for brown rice. The results are summarized in Table 3. It seems from this table quite clear that thiamin deficiency definitely tends to produce the downgrading of fighting capacities in 83.3 per cent of these birds. In many cases this effect began to become obvious from the first test fight (one week after white rice was substituted for brown rice). While there were no definite signs of beriberi or other disease symptoms attributable to thiamin deficiency these birds began to show the tendency of increasing weight and become in-

TABLE 3

Type number of fighting pattern	Before administration of white rice				After administration of white rice				After restoration to standard diet			
	Experimentals		Controls		Experimentals		Controls		Experimentals		Controls	
	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.
I	4	13.33	3	10.00			3	10.00			3	10.00
II	4	13.33	3	10.00			3	10.00			4	13.33
III	3	10.00	4	13.33	1	3.33	4	13.33	1	3.33	4	13.33
IV	3	10.00	4	13.33			4	13.33	1	3.33	3	10.00
V	4	13.33	4	13.33	1	3.33	4	13.33	2	6.67	5	16.67
VI	4	13.33	4	13.33	1	3.33	5	16.67	2	6.67	6	20.00
VII	4	13.33	4		1	3.33	5	16.67	2	6.67	5	16.67
VIII	4	13.33	4		1		2	6.67	1	3.33	0	
IX					8	26.67			6	20.00		
XII					7	23.33			7	23.33		
XIII					10	33.33			8	26.67		
Total	30		30		30		30		30		30	

active, crouching in a corner of the living cage most of the day, even in the fighting pen, paying little attention to the presence of other birds, not only making no attack on other birds, but even when attacked, they either defended themselves feebly and only of a very short duration, or jumped out of the pen, or lowered their heads and accepted the attack passively and submissively. These symptoms began to show about one week after white rice was substituted. What is more interesting is the fact that the restoration to the standard diet has failed significantly (84 per cent) to restore the fighting propensity and capacities of animals.

In this as well as in the previous and following experiments the controls show little changes from their original type numbers.

3. *Experiment III*

In this experiment 60 poor male fighters were used as was the case in Experiments I and II, half as experimentals, half as controls. The experimental procedure is identical in every respect with that of Experiment I except that instead of additional thiamin 5 grams of raw and dry powered egg white was added daily to the standard diet. The results are shown in Table 4. On examination of the table it indicates that protein in the form of powdered egg white also tends to improve the fighting capacities or to upgrade the fighting types of the animals (50 per cent as against 10 per cent of the controls). In our record the effect of additional protein on fighting behavior appears from 8 to 10 days after the commencement of the additional food. The retention of the effect of egg white on fighting capacities is still very definite some three weeks after its withdrawal and comparable with the retention of vitamin B_1.

D. Summary and Discussion

Three experiments were preformed, using Japanese Grey Quails as subjects. The results indicate that (*a*) thiamin and protein in the form of powdered egg white tend to upgrade the fighting types of the birds whereas deficiency of thiamin as a result of feeding white rice tends to downgrade the fighting types.

In the following article (Study III) we report that the male sex hormone is also a potent factor in affecting fighting behavior. What are the underlying physiological mechanisms through which nutritional and hormonal factors produce changes in fighting behavior? Do the nutritional factors in some way influence the output of endocrine secretions which in turn change the picture of fighting behavior? Or, do both nutritional and hormonal

TABLE 4

Type number of fighting pattern	Before administration of egg white				After administration of egg white				After Restoration to standard diet			
	Experimentals		Controls		Experimentals		Controls		Experimentals		Controls	
	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.
I					2	6.67			1	3.33		
II					3	10.00			2	6.67		
III					3	10.00	1	3.33	2	6.67	1	3.33
IV					2	6.67	1	3.33	1	3.33		
V					4	13.33	1	3.33	4	13.33	1	3.33
VI									1	3.33	1	3.33
VII					1	3.33						
VIII												
IX	10	33.33	10	33.33	5	16.67	10	33.33	5	16.67	9	30.00
XII	9	30.00	8	26.67	6	20.00	7	23.33	7	23.33	9	26.67
XIII	11	36.67	12	40.00	4	13.33	10	33.33	7	23.33	10	33.33
Total	30		30		30		30		30		30	

factors tend to work in the same way, that is, to increase metabolic activities so as to energize or vitalize the organism? Work is under way to test the effects on fighting behavior of (*a*) certain recent testoid products which have been claimed to possess little virilizing property while retaining the property of increasing protein metabolic activities; (*b*) other vitamins; (*c*) certain products of the amphetamine family, and (*d*) tranquillizers. Results of this study, it is hoped, may throw some indirect light on this problem.

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STUDIES ON THE BASIC FACTORS IN ANIMAL FIGHTING: III. HORMONAL FACTORS AFFECTING FIGHTING IN QUAILS*

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ZING YANG KUO

A. PROBLEM

In the present report we deal with several experiments on the effects of the gonads on fighting in the Japanese Grey Quails.

B. GENERAL PROCEDURE

Throughout the following experiments we followed strictly the prescriptions of standard diet (except that greens were substituted for orange juice) and standard weight as described in the second article of this series, the exact procedure of isolation and training as described in the fourth article. The terms "good fighter" and "poor fighter" adopted in this report as well as in other reports of this series refer to the animals which could be trained and become one of the first eight types of fighting pattern (good fighters) and to animals which belong to Types IX, XII, and XIII (poor fighters) (see Table 1 in the first article of this series).

C. THE EXPERIMENTS AND RESULTS

The following experiments were performed on a large number of quails:

1. *Experiment I*

To compare sex differences, 50 isolated male and 50 isolated female quails of the same age (8 months) were trained for fighting. The results are shown in Table 1. It will be seen that 56 per cent of the males could be trained to become good fighters whereas only 12 per cent of the females could

TABLE 1
NORMAL BIRDS

Sex	Male	Female
Total number trained	50	50
Number of good fighters after training	28	6
Percentage	56%	12%

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be successfully trained. In gambling in South China, gamblers never used female quails for fighting. This is justifiable as only such a small percentage of female birds can become good fighters.

2. *Experiment II*

In this experiment 40 isolated males and 40 isolated females were gonadectomized at the age of two months. Six months after the operation, the birds were trained according to the standard procedure. The results of the training are summarized in Table 2. In this table two facts appear to be quite definite: (*a*) Only 25 per cent of the gonadectomized males could be trained to be good fighters as against 56 per cent of the normal males. This seems a clear demonstration of the potent effect of testiculer secretion on fighting. (*b*) Ten per cent of the gonadectomized females as against 12 per cent of the normal females could be successfully trained. It seems that the ovaries are not necessarily connected with fighting behavior in the quails in either way.

TABLE 2
RESULTS OF GONADECTOMY

Sex	Male	Female
Total number gonadectomized birds before training	40	40
Number of good fighters after training	10	4
Percentage	25.00%	10%

3. *Experiment III*

In this experiment, two weeks after gonadectomization the birds received a grafting of testicular tissues on both sides of the body under the skin similar to the common practice in grafting in the chicks. The grafted tissues were taken from the testes of another quail about two months old. From the eighth month on the animals went, as usual, through a period of training and test fights. After the test fights were completed, the birds were operated to examine the conditions of the grafted tissues. Only those birds in which the grafts were successful were recorded in Table 3. It will be seen from this table that the grafted tissues seem to assert a great deal of influence on the fighting patterns of the quails. However, it appears that the effect of the grafted tissues is relatively milder than that of the intact testes as a comparison of Tables 1 and 3 will show. Such a difference may be explained, in part, at least, that practically in every case, the grafted tissues are much

TABLE 3
RESULTS OF INPLANTATION OF TESTICULAR TISSUES AFTER GONADECTOMY

Sex	Male	Female
Total number trained	40	40
Number of good fighters after training	20	12
Percentage	50%	30%

smaller than the intact testes. It is highly probable also that the grafts may not function as efficiently as the original testes even though the amount of apparently functional tissues may be about the same in both cases.

4. *Experiment IV*

In this experiment 5 mg. of testosterone propionate was injected every other day to 50 normal males and 50 normal females one week before the training took place. The results are given in Table 4. It is to be observed that there is a much greater percentage of good fighters in both cases produced by training after the treatment of testosterone propionate than that of the untreated birds (see Table 1). One significant fact must be mentioned here. In our investigation of several hundred grey quails the average length of time required to train a good fighter was about 35 days. But with testosterone treatment the average time was reduced to about 25 days.

TABLE 4
RESULTS OF INJECTION OF TESTOSTERONE PROPIONATE TO NORMAL BIRDS

Sex	Male	Female
Total number trained	50	50
Number of good fighters after re-training	42	17
Percentage	84%	34%

5. *Experiment V*

The purpose of this experiment was to determine whether or not testosterone will improve or upgrade the poor fighters. Thirty male and 30 females both belonging to fighting Types IX, XII, and XIII were employed. The treatment of testosterone propionate is the same as in Experiment IV. One week after the treatment the birds were retrained and tested for fighting ability. The results are presented in Table 5. It will be seen that 63.33 per cent of the male poor fighters and 40 per cent of the female poor fighters have been upgraded, that is, changed into good fighters. In our records 67 per cent of these upgraded remained "good fighters" four weeks (the last test fight given) after the administration of testosterone was stopped. While some of the most recent testosterone preparations have been claimed to have prolonged effects (as long as 35 days after injection) at the time when our investigation of the hormonal effects on fighting was carried out, the effect of testosterone by injection was rather of relatively short duration (in our own experience, two to five days). The fact, therefore, that the upgraded birds remained good fighters four weeks (or longer) after the discontinuation of the testosterone treatment must be explained by some other factors than sex hormones. We are inclined to the view that habituation plays rather an important part in such cases. Moreover, habituation may also explain, in part, at least, the fact that only 19 out of 30 poor male fighters and 12 out of 30 poor female fighters could be upgraded by the treatment of testosterone propionate.

TABLE 5
RESULTS OF INJECTION OF .5 MG. TESTOSTERONE PROPIONATE TO POOR FIGHTERS

Sex	Male	Female
Total number of poor fighters	30	30
Number of good fighters after re-training	19	12
Percentage	63.33%	40%

6. *Experiment VI*

In Experiment II we gonadectomized the birds of both sexes before training for fighting. In this experiment gonadectomization was performed after the birds had been trained to be good fighters. After the animals had fully received from the effects of the operation they were given five test fights as usual. The results are shown in Table 6. The result seems to be another

TABLE 6
RESULTS OF GONADECTOMY AFTER TRAINING

Sex	Male	Female
Number of good fighters before gonadectomy	30	20
Number of good fighters after gonadectomy	5	16
Percentage left after gonadectomy	16.66%	80%

clear-cut demonstration of the importance of the male gonads in fighting while the ovaries seem to be a relatively ineffective factor. Whether or not the ovaries have an inhibitory effect on fighting as has been previously reported by other investigators must be determined by a more thorough and systematic study than what had been done before of which the writer is aware.

7. *Experiment VII*

After Experiment VI was completed some one in our laboratory brought up the question whether the changes in the fighting pattern in this experiment could be explained as a result of operation strauma rather than gonadectomization. To eliminate such a possibility we made blank operations, that is without removing the gonads on other 40 good fighters, half males, half females, other aspects of experimental procedures are the same as Experiment VI. The results are shown in Table 7. It seems from an inspection of the table that operation strauma has little effect on fighting.

TABLE 7

Sex	Male	Female
Total number of good fighters before operation	20	20
Number of good fighters after operation	18	19
Percent left after operation	90%	95%

8. *Experiment VIII*

The results of the above experiments led us to the view that since male hormone has a definite influence on the fighting pattern of the quail the size of the testes must also correlate with the fighting ability of the animal. To verify this view we decided to weigh the testes of 195 male birds, 100 of which were poor fighters, the other 95 good fighters. The operation was done about the age of 1½ years when these birds were no longer needed for experiments. The results are tabulated in Table 8. An inspection of the table reveals the fact that not only the average weight of testes is much heavier in the good fighters group (1.11 grams) than the poor fighters group (0.81 grams), but also the weight range as well as the difference in the distribution of weight in upper and lower ranges are decidedly in favor of the good fighters group.

TABLE 8

Good fighters		Poor fighters	
Weight in grams	No. of birds	Weight in grams	No. of birds
1.35	10	0	
1.25	15	0	
1.15	18	0	
1.05	28	8	
0.95	17	26	
0.85	8	31	
0.75	0	8	
0.65	0	12	
0.55	0	7	
0.45	0	8	
Total number of birds	95	Total number of birds	100
Average weight of testes	1.11	Average weight of testes	0.81 gr.
Range	1.35 gr. to	Range	1.05 gr. to 0.45 gr.

D. SUMMARY AND DISCUSSION

In this report, several experiments were related all dealing with the male sex hormone as a potent factor in behavior patterns of fighting in the Japanese grey quail. By comparison of sex differences, by gonadectomization both before and after training for fighting, by grafting of the testicular tissues, by injection of the androgenic steroid, and by comparison of the testicular weights between good and poor fighters all the results point to the same conclusion that the male hormone asserts a decisive influence on animal fighting not only qualitatively and also quantitatively whereas the ovaries have a rather negligible role to play in this matter although the female hormones require a more extensive investigation before definite conclusions can be drawn.

However, several important questions still remain unsolved. How does the androgenic agent work in connection with animal fighting? What are the physiological mechanisms? Is it a side effect of enchanced virility or does it improve fighting as a result of the enhancement of anabolic functions? How is it related to or compared with such nutritional factors as thiamin and other vitamins and protein, etc.? What is the relative importance between such physiological factors and the developmental and environmental factors as reported in the next article? Our investigations so far have clearly demonstrated that developmental, environmental (social or interindividual), and physiological factors are interdependent and interwoven in their action in the control of animal fighting, without one factor, the other two may fail to function effectively. We shall take up the question of relative importance or relative potency of these three factors, physiological, developmental, and environmental in the next article.

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STUDIES ON THE BASIC FACTORS IN ANIMAL FIGHTING: IV. DEVELOPMENTAL AND ENVIRONMENTAL FACTORS AFFECTING FIGHTING IN QUAILS*

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A. PROBLEM

In this report we deal with four specific problems: (*a*) The effects on the fighting behavior of the grey quails of several different early environmental conditions (isolation vs. group life, and specific training for fighting vs. no training) under which the animals have developed. (*b*) The relative importance, as a basic factor in fighting behavior, between the developmental factors (isolation, group life, and specific training) and the physiological factors (vitamins and hormones). (*c*) The relative importance between food and sex drives on the one hand and specific fighting training on the other, and (*d*) the immediate environmental settings which may tend to facilitate or inhibit actual combat. All the birds used in the experiments were male grey quails.

B. METHOD

1. *Experiment I*

This experiment was designed to find out the effect on the fighting behavior of the Japanese quails, of different environmental conditions in early development, specifically, (*a*) the effect of living in isolation, (*b*) living in a group (group life) with other quails, and (*c*) the extent of the effect of special training for fighting on those brought up in isolation and on those brought up in group life. Four groups of 40 birds each were employed. In Group 1 all the birds were isolated as soon as they were matched and never allowed to come into contact with or see another quail except during training and test fights. In the second group the animals were isolated as Group 1, but had not been trained for fighting. In Group 3 the animals were placed in group life in a flock with other non-experimental quails and were never separated from the flock except that each day during feeding time each bird was taken out to be fed in the fighting pen and that during the training and

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test fight period the bird was isolated as those in Groups 1 and 2. In the fourth group the animals were treated exactly the same way as the third group except that there was no training for fighting.

a. Training for fighting. Part of the training procedure described below was adopted from the methods used by quail gamblers. Beginning from eight months old, training was initiated. It lasted for two months. During the training period the animal (whether it was brought up in group life or in isolation) was housed in a cage without any opportunity to hear or see another quail. The animal was kept in a constant hungry state by reducing about 1/3 of its daily rice (brown) intake (on average about one level tablespoon) and food was given at the exact hour of the day. Water was available all the time in the living cage. At feeding the subject was brought into the fighting pen which was made of wood and circular in shape with a wall 14″ high, and 2′ in diameter. In the pen while the animal was devouring its food (standard food as described in the second article of this series) the food was suddenly withdrawn and it had to search and wait for food for five minutes. After it had hurriedly pecked 3 or 4 grains of rice, food was again withdrawn and another five minutes of searching and waiting. This procedure was repeated over and over again until the animal's hunger was about 2/3 satisfied judging by its normal food intake. The animal was then put back to its cage until next day. This lasted for one week. From the eighth day on, instead of withdrawing the food, while the subject was eating, another equally hungry but non-fighting quail (both males and females were used) was introduced to the pen to eat from the same, small (not big enough for two quails to eat together) food dish. Both birds began to struggle for the food dish. At this moment the experimenter quickly took the experimental bird from the food dish, holding it firmly and letting it watch the other bird eating. He moved the bird slowly toward the food dish, but when it is so near that the bird in his hand might be able to reach the food, he moved his hand with the bird away from the dish. This back and forth slow motion of the hand with the bird which was struggling violently to free itself from the hand to reach the food was repeated five times, and then, the bird was loosened from the hand, rushed to peck food. After eating 3 or 4 grains of rice, the animal was again quickly picked up in the hand, and the same back and forth motion of the hand was repeated as before. The food dish was so small that when occupied by the other bird, the experimental bird, after being freed from the experimenter's hand, often had to push away or peck the other bird in eating in order to reach the food. In majority of cases the experimental bird pecked the eating bird instead of pushing it away. But if the subject preferred pushing instead of pecking, the experimenter took another non-fighting bird in his other hand, holding it near the experimental bird still struggling to free itself from the experimenter's hand, so as to elicit pecking at the bird held in the other hand of the experimenter.

When at the food dish the experimental bird began to peck the other bird, the peaceful non-fighting, hungry animal usually kept on eating until or unless the pecking was done with great violance in which case the pecked bird ran away from the food dish and the experimental animal had the full possession of the food. In case severe pecking had discouraged the other bird from going to the food dish again, it was replaced by another hungry non-fighting quail. The same procedure was carried out every day until the experimental bird had formed the habit of attacking any quail whenever it was seen eating food in the dish or in the other hand of the experimenter. After this habit was formed, a great many experimental birds would gradually tend to attack any quail in sight even when there was no food in the pen and they were not hungry. Once in every other day a dummy bird about the same size and same color of a quail was introduced to test whether the experimental animal would attack the dummy as it did a real quail. At the end of two months, those quails which failed consistently to peck the other bird either in the food dish or in the hand of the experimenter were put back to their cages without further experiment and the mark "non-fighter" was tagged to the cages.

This experimental procedure was repeated every day for two months. After this the animal was given five test fights at a three-day interval. In the test fights the trainee was placed face to face with another trained fighter in the fighting pen as well as outside the pen. After the combat had started and the trainee was observed to be in danger of being defeated, the fighting was immediately interrupted and both birds were put back into their living cages. In the next test a weaker fighter was placed to match him. When the trainee was on the way to win the fight, we let the combat go on until a definite decision was reached. After the last test fight (the fifth) the fighting type of the trainee (based on the last test) was classified according to the type numbers in Table 1 of the first article of this series and the animal was put in the living cage on reserve for further experiments.

2. *Experiment II*

The aim of this experiment was to compare the differences of effects of thiamin and testosterone on fighting behavior of quails brought up in different environmental conditions. There were eight groups of birds of 40 each used in this experiment: (*a*) Isolation and thiamin; (*b*) isolation and thiamin and training; (*c*) isolation and testosterone; (*d*) isolation and testosterone and training; (*e*) group life and thiamin; (*f*) group life and thiamin and training; (*g*) group life and testosterone; and (*h*) group life and testosterone and training. Administration of thiamin and testosterone and other aspects of the experimental procedure were identical with those described in the previous reports of this series.

3. *Results*

The results of Experiments I and II are summarized in a composite table (Table 1). Two main columns in this table require explanatory notes. Under the column "Intensity of Combat" we divide the intensity of fighting between two birds into three grades, namely, first, second, and third degrees. The first degree of fighting consists of merely pecking at the opponent's body with little or no injury to the body of either combatant. The second degree is of more severe intensity of fighting; it consists of pulling off, as a result of intensive pecking, of feathers from the body, especially the head and the neck of either or both combatants. The third degree is the most severe and intensive fighting which results in the tearing off the skin of the head or neck or both of either or both birds, in addition to pulling off the feathers, which may cause hemorrhage in many cases. As stated in the previous articles (II and III of this series) "good fighters" include birds of the first eight types (except Type IIIA) listed in Table 1 of the first article. However, the column "poor fighters" in Table 1 of the present article includes several other types of behavior patterns in addition to Types IX, XII, and XIII listed in Table 1 in the first article, such as Types X, XI, XIV, XV, XVI, and XXI. These last six types of behavior patterns are observed only in quails brought up in a group before training for fighting took place. In our records, nearly 50 per cent of the birds living in groups and listed as "poor fighters" after training belonged to these six types whereas none of the "poor fighters" in the isolation groups displayed behavior pattern which could be included in any one of these six types. This is one of the striking differences of fighting patterns between birds raised in isolation and birds raised in a group with other quails.

One word must be added in passing. The general term "good fighters" has been used throughout this series of articles. Another term "Ferocious and durable fighters" will be presently introduced in this report as well as in some of the subsequent reports. "Good fighters" include all the animals which would fight under the circumstances described in Table 1 of the first

TABLE 1

Environmental conditions under which the birds were brought up	No. of birds em-ployed	Types of birds produced by different developmental conditions				Intensity of combat						Duration of combat							
		Good fighters		Poor fighters		First degree		Second degree		Third degree		5 to 15 Sec.		16 to 30 Sec.		1 to 2 Min.		2 to 20 Min.	
		No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.
Isolation only	40	10	25%	30	75%	8	80%	2	20%	—	—	10	100%	—	—	—	—	—	—
Isolation & Training	40	24	60%	16	40%	3	12.5%	16	66.66%	5	20.84%	1	4.13%	5	20.87%	15	62.5%	3	12.5%
Isolation & B_1	40	19	47.5%	21	52.5%	12	63.11%	7	36.84%	—	—	15	78.85%	4	21.05%	—	—	—	—
Isolation & Testosterone	40	20	50%	20	50%	15	75%	5	25%	—	—	14	70%	6	30%	—	—	—	—
Isolation & Training & B_1	40	30	75%	10	25%	3	10%	19	63.33%	8	26.66%	2	6.66%	6	20%	20	66.67%	2	6.67%
Isolation & Training & Testosterone	40	32	80%	8	20%	6	18.75%	20	62.5%	6	18.75%	3	9.38%	9	28.12%	17	53.12%	3	9.38%
Group life only	40	4	10%	36	90%	4	100%	—	—	—	—	4	100%	—	—	—	—	—	—
Group life & Training	40	10	25%	30	75%	9	90%	1	10%	—	—	7	70%	3	30%	—	—	—	—
Group life & B_1	40	7	17.5%	33	82.5%	6	85.7%	1	14.3%	—	—	5	71.4%	2	28.6%	—	—	—	—
Group life & Testosterone	40	8	20%	32	80%	7	87.5%	1	12.5%	—	—	6	75%	2	25%	—	—	—	—
Group life & Training & B_1	40	12	30%	28	70%	11	91.7%	1	8.3%			8	66.6%	4	33.33%				
Group life & Training & Testosterone	40	13	32.5%	27	67.5%	11	84.6%	2	15.4%			10	76.9%	3	23.1%				

article. Such animals may be just ordinary fighters which fight without much ferocity and would not or could not stand a long fight, or they may be fierce fighters and endure the fight for a much longer length of time. We shall refer this later group of good fighters as "Ferocious and durable fighters."

Examination of Table 1 reveals several important differences in fighting behavior between birds raised in isolation and those raised in a group: (*a*) isolation alone, that is, without training, produces more "good fighters" (25 per cent) than socialized birds (raised in group) (10 per cent). (*b*) Training for fighting helps to boost the good fighters from 25 per cent to 60 per cent in the case of birds raised in isolation as against from 10 per cent to 25 per cent of the birds in group life. (*c*) Without training, thiamin and testosterone boost up the percentage of good fighters in the isolation groups to 47.5 per cent and 50 per cent respectively whereas the percentage of good fighters in the case of birds brought up in group life are increased by the aid of thiamin and testosterone only up to 17.5 per cent and 20 per cent respectively. And (*d*) while with the aid of thiamin and testosterone the percentages of isolated birds with training were boosted up from 60 per cent to 75 per cent and 80 per cent respectively, the increases by thiamin and testosterone of the birds living in group and with training are only from 25 per cent to 30 per cent and 32.5 per cent respectively.

If we take the increase in the number and percentage of good fighters as an index we find that in the case of isolated birds, training has increased by 35 per cent (that is from 25 to 60 per cent) while thiamin and testosterone propionate increased only 22.5 per cent and 25 per cent respectively. On the other hand, in the case of birds living in a group the increases of good fighters as a result of training and administration of thiamin and testosterone are only 15 per cent, 75 per cent, and 10 per cent respectively.

The above facts all tend to stress the importance of the early developmental conditions in shaping the behavior patterns of the animal with regard to fighting.

When we take intensity and duration of combat (see Table 1) into consideration, we find that when isolation and training are combined, the birds (good fighters) fight more ferociously and endure fighting much longer than isolated birds without training (although they are grouped together as good fighters). Thiamin and testosterone are helpful in increasing the intensity and duration of fighting up to a certain point, but fail completely to produce the most ferocious fighters who could inflict the third degree of injury on their opponents and who could endure fighting as long as two minutes or longer unless they are combined with training. In the case of birds brought up in group life, their fighting quality does not go beyond the first degree (that is, pecking without pulling off feathers from the opponents' head or neck) and their fighting duration always lasts below 5 seconds as our records indicate. In such birds, training, thiamin, or testosterone alone, or training in combination with thiamin or with testosterone may turn a very small number of these animals into second degree fighters (that is, pulling off feathers from the opponents) and make them endure fighting a little longer, but fail entirely to produce a single fighter who can inflict the third degree injury on its enemy and who can endure fighting longer than 30 seconds. This demonstrates that, important as it is in bringing up animal fighters, the effect of training is greatly minimized unless it is combined with isolation. In other words, isolation and training, when combined, become the most important and most potent factor in producing ferocious and durable fighting birds. Treatment of thiamin or testosterone is effective only when it works together with isolation, especially isolation plus training. Group life may be able to produce a small number of "good fighters" of a sort but rarely, if ever, produces, even with the help of training, thiamin, and testosterone, ferocious and durable fighters.

From the results of these two experiments it becomes clear that early developmental conditions are primary factors in bringing up fighters in quails. Group life produces very few good fighters, and practically, none of the ferocious and durable fighters notwithstanding the help of thiamin, testosterone, and training. Isolation in combination with special training not only produces a much larger number of good fighters but is also the only factor which is capable of producing fierce and durable fighters. In both cases, thiamin and testosterone function merely as contributory factors.

4. *Experiment III*

It is a well known fact that competition over food or over sex is an inducing factor to fighting. In our training for fighters we also used food and starvation to elicit attacking reactions. In the case of training dog fight, sex was also used as a stimulus for provoking fighting (see Article VII). Two questions arise: (*a*) Would an animal, once trained as a good fighter, start to fight not only in the absence of food or sex, but also ignoring the presence of the opposite sex or food? (*b*) Is more intensive and more durable fighting produced as a result of competition over food or sex, or as a result of specific training? To put these two questions to a test the following experiment was performed:

1. Fifty trained and isolated males which had formed a habit of initiating

the attack in the presence of another quail were selected. They were matched according to their fighting type and divided into 25 pairs. Five days before each pair was put into the fighting pen to face each other, each bird was separately fed daily in the pen. For 12 consecutive days the pair was made to have an encounter with each other in the pen in which the food dish and a female quail were also present. The positions of the food dish and the female were so arranged that when the two males were introduced into the pen to face each other, the food dish and the female would always stand between them. The reactions of the two males were then recorded. Thus each pair had 12 encounters in the pen, making the total encounters of all 50 male fighters 300.

2. Another 50 males, totally untrained, brought up in group life, and sexually experienced were selected and paired as in Test 1. They were allowed to perform copulation with females in the fighting pen five days before each pair was made to have encounters with each other. The rest of the test is the same as Test 1 except that during the encounters only a female was placed between the pair; there was no food dish in the pen. During the test period, these males were not allowed to have access to females except in the fighting pen.

3. A third group of 50 totally untrained socialized males were employed. The experimental procedure of this part is exactly the same as Test 1 except that no female quail was present during the time of encounters.

In these three tests there was some difference in the schedule of feeding. In Tests 1 and 3 feeding was made inside the fighting pen after the encounters of the pairs. In Test 2 the birds were fed outside the pen before the encounters.

The results of this experiment are shown in Table 2. It appears quite clear that combat as a result of competition over food or over sex occurs very much less frequently than the incidences of combat as a result of special training (over food, 43 per cent and over sex, 29 per cent as against 95 per cent with the trained fighters). Furthermore, in no case is fighting over food or over sex comparable with the combat of trained fighters either in the intensity of fighting or in the duration of combat. In our observation the great majority of untrained males, when they were placed in the fighting pen they immediately rushed to the food dish or the female in sight as the case might be, ignoring the presence of the other male. When both males were so close as to interfere with eating or copulation, one of them would peck at the other which would retreat or run, and the attacking bird then, had the full possession of the food dish or the female. The attacking or pecking was so

TABLE 2

Causes of combat	Total No. of animals	Total No. of encounters	Incidences of combat		Intensity of combat						Duration of combat							
					First degree		Second degree		Third degree		5 to 15 Sec.		16 to 30 Sec.		1 to 2 Min.		2 to 20 Min.	
			No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.	No.	P.C.
Over food	50	300	129	43%	120	93.03%	9	6.97%			123	95.35%	6	4.65%				
Over sex	50	300	87	29%	81	93.1%	6	6.89%			75	86.21%	12	13.79%				
Due to training	50	300	285	95%	12	4.21%	255	89.47%	18	6.32%	12	4.21%	132	46.32%	111	38.95%	30	10.53%

mild that we rarely noticed any loss of feathers. Even in a very small number of cases when the attack was answered by counter attack, the combat was very mild and brief indeed. On the other hand, when two trained birds were put in the pen, their first act was to attack any bird in sight and close to it, whether it was the matched opponent or the female. In no case did we observe any of the trained fighters to be attracted by the presence of a hen quail as such. To them she was either an opponent to be attacked, or to be ignored. As to the presence of the food dish in the pen, only 5 per cent of the total 300 encounters of 25 pairs of trained fighters were attracted by it and preferred eating to fighting although all of them were starved for 24 hours, and when the encounters were made it was their feeding time, and the pen was their recent feeding place.

5. *Experiment IV*

We have demonstrated in the previous experiments that developmental factors such as isolation, group life, and training determined whether a given animal was to grow up as a ferocious and durable fighter or a peaceful and non-resistant bird. However, given a trained fighter, whether actual combat would take place depended upon its immediate environmental settings, such as the kind of opponent it was facing and the physical conditions under which the animal was placed. We made this observation during our investigations on fighting in connection with other problems. To verify this observation, the present experiment was performed.

A large number of quails which had been previously employed in other experiments were assembled and classified according to their previous fighting records. The classes are as follows: (*a*) attackers which would always attack another bird in sight, (*b*) defenders which would fight only when attacked by its opponent, (*c*) winners which were victors in previous experiments, (*d*) losers which had been defeated in previous experiments, (*e*) non-resisters which in previous experiments never made a counter attack whenever attacked by other quails.

We paired these birds in different settings and gave each pair four encounters (one a day in four consecutive days) in the fighting pen and recorded whether actual combat took place. The settings were as follows: (*a*) attackers vs. attackers, (*b*) attackers vs. defenders, (*c*) defenders vs. defenders, (*d*) attackers vs. non-resisters, (*e*) winners vs. losers, (*f*) winners vs. winners, (*g*) losers vs. losers.

After this part of the experiment was completed another part was conducted as follows. Two hundred previously employed trained attackers were collected and divided into four groups. One group was tested for actual combat inside the fighting pen where they were trained and had various fighting experiences in previous experiments. The second group was tested outside the fighting pen where the birds had no previous fighting experiences. The third group was tested in a flock where a large number of quails lived together. The fourth group was tested in a house where many different species of birds lived together. In these tests every bird in each group was given two close encounters with another bird of the same group. The observation of the behavior of both birds in each encounter was recorded.

TABLE 3

Conditions under which encounters take place	Total No. of birds	Total No. of encounters	Incidences of combat	
			No.	P.C.
Attackers vs. Attackers	50	100	96	96%
Attackers vs. Defenders	60	120	102	85%
Defenders vs. Defenders	50	100	20	20%
Attackers vs. Non-Resisters	40	80	0	0
Winners vs. Losers	50	100	42	42%
Winners vs. Winners	50	100	94	94%
Losers vs. Losers	50	100	48	48%
Inside fighting pen (trained attackers)	50	100	93	93%
Outside fighting pen (trained attackers)	50	100	28	28%
In flocks (trained attackers)	50	100	5	5%
In inter-species (trained attackers)	50	100	2	2%

Table 3 summarizes the actual occurrences of physical combat during the encounters under different environmental settings as recorded in both parts of the experiments. From the facts presented in this table it seems safe to draw the conclusion that the same trained fighter would react very differently under different social settings (different inter-individual relationships) as well as under different physical environmental settings (inside vs. outside the fighting pen). Fighting would not actually occur unless the reaction of the other animal in the encounter was favorable. Moreover, in the greatest majority of cases under study (95 per cent to 98 per cent), all the past training and past experiences in fighting were lost almost *in toto* once the familiar ground was removed, and the animal was placed in an entirely different physical environment (such as outside the fighting pen) or entirely different social setting. The animal became wild, running and jumping about aimlessly, making no response if it was chased by another bird in the flock or in the interspecies house. Unless the animal was soon taken back to its isolation cage, the loss of the fighting habit would become permanent. When such an animal was allowed to stay in the flock or in the interspecies bird house, it always stayed aloof from other birds showing no sign of interest in female quails in the flock, nor paying any attention to other birds, although it was once a very fierce fighter or attacker and had adjusted itself to the new environment. We had preliminary experiments to find out whether these birds, after a certain period of life in a group could be retrained and brought back to become good fighters again. As our record was not complete, no report will be published. However, the general indication of our record was clearcut and may be mentioned here. We found that after these birds were placed in a social group for more than two weeks, only a very small percentage could be retrained and brought back to become fighters. Even so, these reinstated fighters were no longer as ferocious and as durable as they had been once before.

C. Summary and Discussion

In this report we deal with several experiments to demonstrate the importance of developmental history and the immediate different environmental settings in connection with the fighting behavior of the quails. It was found that (*a*) isolation in early life in combination with special training is a primary factor in producing not only a large number of fighters but also ferocious and durable ones. (*b*) Immediate different physical or social environmental settings tend to either facilitate or inhibit actual combat between birds. (*c*) Nutritional and hormonal factors act merely as contributory factors in promoting fighting behavior. (*d*) Radical changes in the environmental conditions in which the animal is placed tend to demolish, temporarily or permanently, the fighting habit previously formed (this is not applicable to dogs or Siamese fight fish however. See Kuo'). And (*e*) once the fighting habit is well established, food and sex fail to be an effective stimulus when two well trained fighters are brought face to face and on a familiar ground. This last finding is true not only in quails but also in dogs and chickens (see Kuo' Studies etc. VIII and XII).

In our two preceding reports (Kuo' Studies II and III) we had stressed on several occasions the importance of habituation in animal fighting. (We used the term "habituation" neither in the sense of negative adaptation, nor in the Pavlowian sense of extinction of a conditioned reflex. We used it in the sense of the formation and fixation of various degrees of certain types of behavior). Our view can be briefly stated thus:

When the fighting habit or non-fighting habit of an animal is well established, or "habituated," it is difficult to reverse such a habit even with favorable physiological factors and favorable environmental factors (such as special training). A trained fighter would ignore the presence of food (even when hungry) or sex objects and rush to attack or challenge its opponent.

"Habituation," however, had another and much broader sense than a well-established habit. We refer to the *"total behavior make-up" as the result of the developmental history of the animal.* A well-established habit such as fighting may be temporarily or permanently broken down as a result of drastic changes in the environmental settings. And in certain instances a "broken" habit may be reformed under favorable and adequate environmental setting. But when we come to the total *"behavior make-up"* which is also a form of habituation and which is the sum product of the developmental history of the individual, sudden and drastic changes in environmental setting may momentarily upset the behavior pattern of the animal, but as soon as the animal "gets adjusted to" or "settled in" the new environment, its original *behavior make-up* which was acquired during its development returns to dominate the scene although certain individual habits such as fighting (as in the case of grey quails, but not in the case of the dog or Siamese fighting fish —see Kuo: Studies etc. V, VIII, and XX) may become permanently extinct (and here we wish to emphasize the fact that extinction could be very sudden and does not require a slow and gradual process as in the case of Pavlow's conditioned reflex). Thus, when a trained grey quail fighter is transferred from solitary life to live in a group with other quails or with birds of other species it may lose its fighting habit, but its solitary habit and indifference to other animals remain unchanged. A young chick or a young pigeon or a dog raised in isolation, when fully grown-up, will not mingle with other animals of its own species. A kitten raised together with a white rat will treat the rat as its sole companion while adopting a hostile attitude towards another cat. In the subsequent articles of this series we shall have several occasions to note that a Siamese fighting fish, a chick, a quail, a song thrush, or a dog could become a most vicious fighter, or a peaceful animal, or even a helpless victim of aggression, it depends on the way the animal is brought up. The point we wish to emphasize here is that whenever we undertake to investigate any type of behavior (man and animal) we must take into consideration the *total behavior make-up together with its developmental history in its various stages.* Indeed, any study of behavior, be it a case of fighting, learning, social relationships, physiological mechanisms, pathological behavior, or what not, will become fragmentary or even superficial, if we do not put the total behavior make-up of the animal and its developmental history in a proper perspective. For example, we shall not be able to understand why special training for fighting may fail in certain cases, but is successful in certain others, or why the treatment of thiamin or testosterone propionate may succeed or fail to increase the fighting tendency of an animal unless the developmental history of the animal under investigation is adequately known.

The point of view we have been stressing may be roughly expressed by the following formula:

$$\underset{(1)}{(\mathrm{DH} + \mathrm{ST})} + \underset{(2)}{(\mathrm{PE\ Set.} + \mathrm{SS})} + \underset{(3)}{(\mathrm{BCF})} \rightarrow \text{Pred. Beh.}$$

Where: DH = developmental history
ST = Special training (such as fighting, sensory discrimination, etc.
PE Set. = present environmental setting
SS = special stimulus (such as light, or sound, food, or sex, object, etc.)
BCF = Bio-chemical factors
Pred. Beh. = predictable behavior.

Bracket (1) deals with the animal's past, (2) with its present, and (3) with its physiological mechanisms.

It goes without saying that behavior is predictable only when the essential factors in all the three brackets are well-known and understood. And yet, in our behavior studies in the past few decades the developmental aspects were most often neglected. The Watsonian behaviorists and their allies as well as the Pavlowian conditioned reflexiologists have been contented with the over-simplified S - R. formula (S here is equivalent to our SS). The Gestalt school and more recently the ethologists have laid great stress mostly on the present environmental settings. But the neglect of developmental history had made a great many of our behavior studies look fragmentary and void of fundamental understanding of animal behavior. And it is, partly at least, due to such a neglect that a number of investigators of animal behavior have to resort to such concepts as inheritance and maturation or to instrospective or even anthropomorphismic concepts in order to explain their experimental results.

No one can refute the view that every bit of behavior is resolvable into physiological factors, neurological, endocrinological, or otherwise. However, every animal is the creature of its developmental history and reacts to a specific stimulus in a specific environmental setting. Unless all these factors are carefully and systematically worked out we can not hope to have a comprehensive understanding of animal behavior, and our behavior studies will always remain piecemeal and lack systematic coordination.

Our theoretical view as sketched in the above discussion is the direct outgrowth of many years of studies in the development of behavior of the avian embryo, the development of the behavior of the cat in relation to the rat, and particularly in fighting behavior in a number of species. It seems, therefore, appropriate to ouline it here in connection with our report on the developmental factors in fighting.

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RETENTION OF DISCRIMINATION HABITS BY CATS*

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A. PROBLEM

Brogden (1) has pointed out that very little information is available regarding retention of discrimination learning by experimental animals. The purpose of this experiment was to measure the retention of object discrimination habits by cats over the range from 12 hours to 8 days.

B. METHOD

1. *Subjects*

Five young cats, approximately six months of age, were studied. These *S*s had previously been trained on a series of 36 discrimination and transfer problems.

2. *Apparatus*

Testing was conducted in the Wisconsin General Test Apparatus. The stimulus objects were pairs of wooden figures which varied randomly in multiple visual dimensions (e.g., form, brightness, surface area, and thickness).

3. *Procedure*

The cats were tested on two sets of five problems, each set constituting one replication of a 5x5 latin square design. The retention intervals were 12 hrs., 1, 2, 4, and 8 days. Thus each cat was tested on two problems for each of five retention intervals, with the order systematically varied between animals. Approximately 24 hrs. elapsed between the retention test on one problem and the beginning of the next problem. In the first replication the 12 hr. interval was during the day, while in the second this interval was overnight. No difference in retention under these two conditions was found.

The criterion for learning was 20 correct out of 25 consecutive responses, using the noncorrection technique. Savings scores were computed by subtracting number of relearning errors from number of original learning errors and dividing by number of original learning errors.

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C. Results

In Figure 1 the mean and median savings scores for each of the five retention intervals are shown. Each point represents 10 individual savings scores (i.e., five cats with two problems at each interval). The graphs clearly indicate no significant loss in retention over the range from 12 hrs. to 8 days. Since savings scores may give a distorted picture of the original data (e.g., occasional savings of minus infinity), it is more defensible to argue from the data in Figure 2. If there were a progressive loss in retention during the intervals studied, then the lower (relearning) curve in Figure 2 would rise toward the right.

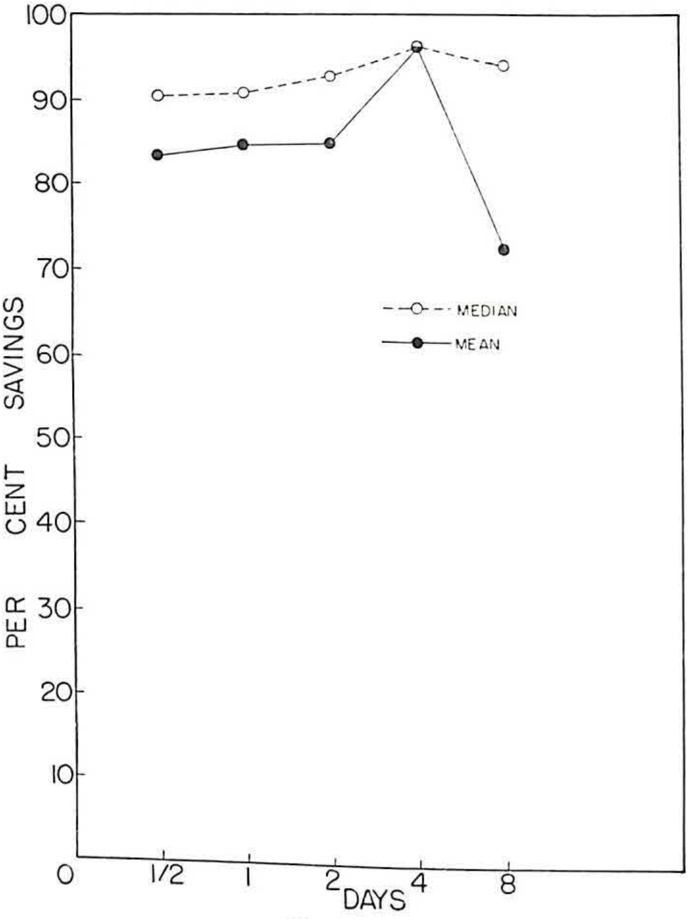

FIGURE 1
Savings Scores As a Function of Test-Retest Interval

There was no significant correlation between number of errors on original learning and relearning across the 10 problems.

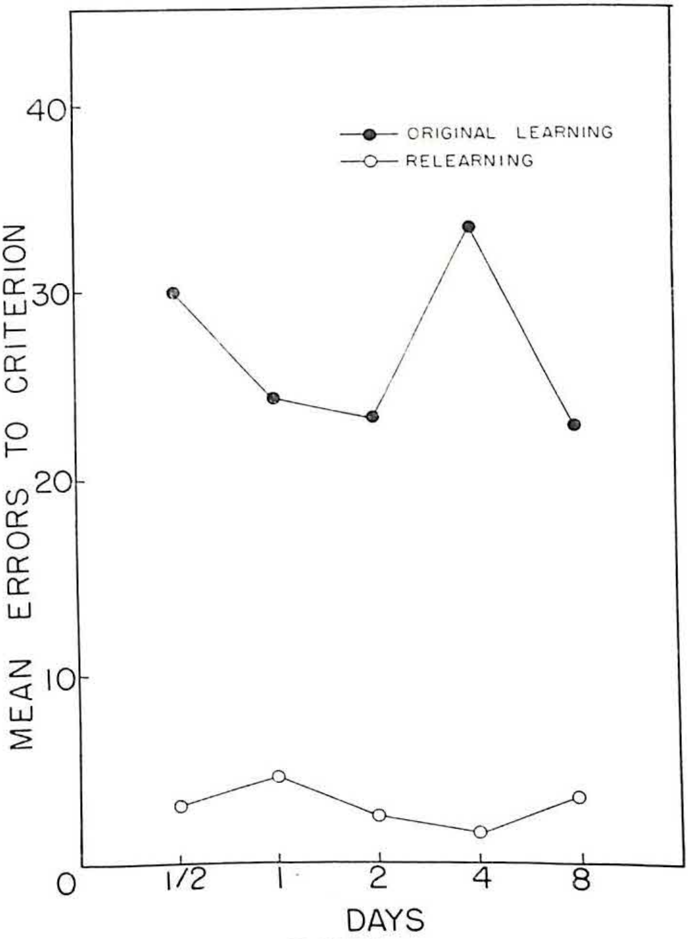

FIGURE 2

Errors to Criterion in Relearning As a Function of Test-Retest Interval

D. Discussion

Neither savings scores nor number of errors to relearn showed any systematic relationship to retention intervals. This agrees with observations of discrimination learning reported by Smith (5), who found around 90 per cent savings for two cats after 7 and 12 months. Similar results with rats have been reported by Lashley (4), up to 14 days; by Jones and Jones (3), up to six months; and by Fields (2), up to seven months.

E. SUMMARY

Five cats were tested for retention of discrimination problems 12 hrs., 1, 2, 4, and 8 days after original learning. No significant relationship was found between retention intervals and either savings scores or number of errors to relearn to the same criterion.

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THE DRIVE-MODIFICATION THEORY OF HUMAN MOTIVATION*

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A. INTRODUCTION

It is probable that the coming era in the development of academic experimental psychology will in due course of time be known as the "age of motivation," just as the past half-century might well be called the "age of learning." Whether or not the incipient stages of such an epoch can be recognized, nevertheless, problems of motivation loom very large on the current scene, in experimental fields, as well as in the more obvious vistas of personality and social psychology. This is not to say, of course, that all psychologies of all ages have not in some manner incorporated a notion of motivation into their systems, albeit occasionally with implicit rather than explicit awareness. Rather it is to say that psychology appears to be entering a phase in which the problem of motivation will occupy the center of the stage, as once psychophysics, and later learning and intelligence testing filled the starring roles.

The term "motivation" is here taken to be a broad term covering all the energizing, instigating, and regulating processes of the organism. It thus refers to many kinds of determinants of behavior which are taken to occur always in complex interrelated systems. As a consequence, it is essential to differentiate the various components of these systems—and the terminology of motivation is bound to include numerous terms. It will be the intent of this article to seek clarification of these components. We shall confine ourselves to human motivation, without attempting at this juncture to treat the results of animal experimentation in terms of the viewpoint to be presented.

Three major problems arise in the study of human motivation.

1. The *structural problem* refers to the levels, or aspects, of organization that may be inferred from the responses of a person. That is, when a person performs an act one may try to assess the processes which were responsible

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[1] Thanks are due to Dr. Boyd McCandless and Dr. John Digman, who patiently read earlier versions of this paper. Their thoughtful comments have been of great assistance, but they are not to be held responsible for the interpretations made in this paper.

for the performance of that response rather than some alternative response. Such determinants may be *temporary adjusting processes*, stemming from the properties of the immediate stimulating conditions, as a function of which the behavior of the organism assumes a specific focus. In the literature of experimental psychology these determinants have most frequently been called "sets," implying only that some immediate tendency has brought about a particular response. At a deeper or more permanent level, one may infer *directing or regulating* processes which cut across many responses, rendering them similar in function, representing rather general orientations to objects, and directing behavior in ways consistent from one situation to another. These determinants are usually called "attitudes." At a still deeper and more permanent level are *those processes which instigate* large, complex, significant, and continuing segments of behavior in quite general ways. To infer these factors, one must look across attitudes to see what functions are subserved by them. These very determinants are usually called "drives" or "motives." Finally, the substratum of motivation rests in the continuing metabolism of the organism which represents the process of originating behavior of any sort, and this may be called the *"energizing" function*, which will here simply be called the problem of "energy."

Much of the history of how motivation has been handled in psychological theory depends upon the level of inference adopted by the theorist and, as a consequence, upon the sorts of technique employed. One may approach the problem of structure either intensively or extensively, by seeking either to obtain data pertinent to more fundamental or "deep" aspects of motivation, or to get information about broader or more inclusive aspects. In either case, one tries to relate observable specific acts of a person to larger patterns which provide a more significant explanation of the acts themselves. Once the inferred pattern is formulated, the question then becomes essentially one of predicting an act or series of acts supposed to be the outcome or manifestation of the inferred process. The more successful one is in inferring deeper or broader levels, the less close the specific act may be to the structural component, i.e., the lower the correlation between specific acts and inferred processes, but, at the same time, the greater the number or variety of acts that will show a correlation.

2. The *functional problem* refers to the inferred properties of a sequence of behavior considered over time, including dominant components, cyclicity, direction, regulation, and outcome. All theories of motivation seek in some manner to specify the dynamic properties of energizing, instigating, and regulating (although with different conceptions of the structural problem

represented by these terms). Perhaps the commonest forms of this interpretation have to do with notions of the varying strength of the components of motivation chosen, whether as a consequence of changes in the concentration or distribution of energy, of tissue-depletion and restoration, of interference in the regular operation of a system (frustration and conflict), or of peculiarities of the relationships among deeper and more superficial levels (e.g., repression and its results, inadequacies of "steering" in relation to "driving" processes, etc.).

There is not space in this article to explore all the diverse facets of this problem, but we shall rather hope to clarify somewhat its character, and to lay a foundation upon which meaningful hypotheses can be constructed.

3. The *chronological problem* refers to the origins of instigating and regulating processes in the biological properties of the organism and their progressive modification and organization through the life-cycle. In short, this problem has to do with the development of the person from a motivational point-of-view. Three major approaches have become familiar in psychology, namely, (*a*) the study of age-to-age changes in the same persons, the chief disadvantages of which are no doubt the great expense involved and the long time it takes; (*b*) the age-sampling technique, whereby one investigates simultaneously persons at representative stages of development, the chief disadvantage of which is the difficulty of establishing continuities in development; and (*c*) the retrogressive analysis procedure, typical of psychoanalysis, where one takes a person at a given stage and seeks to reconstruct his prior history, of which the chief disadvantage stems from the fact that the current organization of a person may in a variety of ways obscure or eliminate the recall of previous events. All of these approaches are widely used and all have much to recommend them, but, perhaps, for testing different kinds of hypotheses. To me, the only sure way to cope with the chronological problem of motivation is to carry out age-to-age studies (at least in the beginning), as shall be clarified later.

In general, it is with reference to this chronological problem of motivation that psychology has made one of its most distinctive contributions to human knowledge, by revealing the role of experience in shaping adult characteristics. One major consequence has been the discarding of simple instinct theory where the distinction between the structural and chronological problems is notably absent. It is also with respect to this third motivational problem that anthropology conspicuously joins forces with developmental psychology to demonstrate that structures inferred at one stage (or in one society) cannot necessarily be assumed to apply to another stage (or society).

Unfortunately, again, limitations of space make it impossible to deal in any detail with chronological problems of motivation, and we shall be forced merely to state a position for which evidence can only be cited in general ways.

B. Theories of Motivation

To clarify further the central intention of this paper, it is pertinent to identify the kind of theory with which it is concerned. To this end, we may recognize that four principal sorts of motivation theory have received serious formulation during the period since psychology began to emerge as an experimental science. For each of these views, certain outstanding names may be listed, although in some cases I may give an association representing my own understanding of the essence of the theory, rather than one fully acceptable to the writer in question. Furthermore, no complete listing will be presented, here, but only typical sources.

1. *Instinct* (Freud, 25; McDougall, 46; Tinbergen, 60)

In this conception, organisms, including man, are said to be innately equipped with the basis for tendencies which determine very large segments of behavior subsequent to birth, including the modes whereby these impulses are directed and expressed; that is, not only the energizing and instigating aspects of motivation, but also much of the directive function is claimed to be inherent in the species. Despite the rejection of this doctrine in simple form, about the only psychologist who has not allowed in some measure for an innate basis for motivation is Kuo (39, 40), who shrewdly and forcefully carried the implications of the behavioristic platform to its logical extreme. In the course of time, it has become clear that the weaknesses of instinct theory lie not so much in postulating innate motivational patterns (or, in another terminology, "unlearned tendencies"), as in failing to explain *how much* of behavior is allocated to inherent biological properties and *what happens* to these properties. This change in point-of-view, as indicated above, will be recognized as the result of research concerned at least indirectly with the chronological problem of motivation, including the relation between heredity and environment. It is now widely accepted in psychology that neither innate characteristics nor experience is more important than the other, but that there are always important relations between them, which must be ascertained for each psychological function analyzed. While, therefore, we can no longer assign the characteristics of adult behavior to instinct, we can, perhaps, attribute their origins in significant ways to it.

2. *Drive-Reduction* (Hull, 31; Dollard and Miller, 15; Brown, 8, 9; Farber, 20; Miller, 50; Seward, 58; Young, 74; Tolman, 61-63; Mowrer, 51; Nissen, 56)

According to this view, organisms are equipped with certain fundamental, innate motivational tendencies—usually linked with internal visceral states—but may also acquire additional tendencies as a consequence of learning. It is not always clear whether the secondary tendencies may operate independently of the primary drive, but, on the whole, it appears that the former are regarded essentially as agencies for the latter. That is, the secondary tendencies would not continue as determinants of behavior without their continued connection in some fashion with or their reinforcement by the primary drive originally present in the learning situation. In relation to the structural problem summarized above, it may be said that the drive-reduction theory considers primary drives to be energizing functions and secondary drives to be regulative functions, without allowing clearly for the additional property of instigation, to be elaborated below. A weakness of this kind of theory lies in its heavy reliance upon research with animals, with the result that its applicability to human behavior has not convincingly been demonstrated, despite notable efforts by men like Dollard and Miller (15), and Mowrer (51). Further weaknesses lie in the inadequacy with which the concept of primary drive has been formulated (for instance, it does not seem clear whether there is one "drive state," or several drives; in the fact that the physiological components of a primary drive have been very vaguely treated); and in the inclination to confuse very temporary and trivial incidents with the long-continuing sequences of development—i.e., insufficient attention to the chronological problem of motivation.

3. *Functional Autonomy* (James, 32; Watson, 66-67; Woodworth, 72; G. W. Allport, 1; Hebb, 28)

In this position, any acquired tendency is regarded as a self-sustaining, independent determinant of behavior, with the consequence that no simple list of drives or motives can be formulated. While not denying that the course of development originates in basic, innate properties, this kind of theory sees them as supplanted by whatever learned mechanisms may currently characterize the individual. In effect, this approach assigns the dominant role in motivation to the directive function, or at least sees no necessity to account explicitly for a difference between the activating and regulative aspects. One weakness, therefore, lies in this diffuse picture of motivation, whereby, in effect, any determinant of behavior is much the same as any other. One is likely to perceive the problem of assessing motives, in this theory, strictly in terms of the immediately measurable properties of the situation where behavior occurs and thus to infer only superficial structures.

4. *Drive-modification* (Murray, 55; Murphy, 52-53; Maslow, 48; Horney, 30; McClelland, 45)

Finally, a type of theory is starting to appear in which the problems of motivation are considered to be very much more complex than in the preceding three kinds of theory. From this standpoint, motivation has to do with the deep-lying, total system of processes which instigate, sustain, and direct behavior. Motivation depends not only upon the inherent biological properties of the organism, but also upon the changes and organization of these materials during the course of development in relation to the sociocultural context. Thus, the cross-sectional problems of analyzing motivation (the structure of motivation at any given time) differ from the longitudinal problems (the sequences of function and modification over developmental time). This type of theory seeks to allow not only for innate determinants but also for the effects of learning, not only for energizing but also for instigating and regulative functions, not only for a single class of motives (i.e., visceral), but also for whatever other classes would be demanded for an adequate accounting of the total integrated system of personality. The general picture that finally appears makes motivation neither primarily an ongrowing of innate properties, nor primarily produced by learning, but a fusion of both sets of influences. Thus, adult motives are seen not as the same as those of infants, nor is there considered to be a sort of superimposition of learned tendencies upon underlying visceral states, but rather the innate "raw materials" become organized and changed as a function of experience—the degree of change depending upon the character both of the biological raw materials and also the character of learning, so that some motives ("psychogenic," "social," "acquired") represent very great modification, whereas others ("biogenic," "visceral") represent, on the average, less change from the original drives. The greatest weakness of this fourth conception of motivation lies in the paucity of acceptable empirical evidence. In effect, voluminous case-history material is demanded, utilizing clinical techniques on *normal* people.

The remainder of this paper will be concerned with an explication of the essential features of drive-modification theory, including a comparison in certain respects with drive-reduction theory. Some of the recent research in human motivation will be cited as it bears upon these points. Since, in terms of drive-modification theory, motivation covers a large number of psychological phenomena frequently kept separate, the pertinent literature is extensive; we can here only make all-too-limited selections from it. As indicated above, we deem McClelland and his associates to be contributing to the drive-modification point-of-view, although the affective-arousal version differs in several respects from the present statement. The chief divergence lies in the assumption that positive and negative affective processes lie at the root of motivation, for in the drive-modification theory autonomic processes constitute only one component of motivation, as we shall see.

C. Energizing

In order to clear the way for a consideration of the major features of drive-modification theory, we may first dispose of the energizing level of inference.

The general metabolism of the organism, with its attendant physiological and neural processes, provides the energy of drive- and motive-systems. This basic aspect of motivation may be regarded—at least at the present stage of psychological knowledge—as a problem for physiology and neurology. That is to say, the energizing function may be accepted simply as a necessary postulate, without making any assumptions regarding the biology of energy, or changes therein over the life-cycle, although it is not necessarily to be considered as a constant. The psychology of motivation is not concerned with the properties of energy itself, but rather with the forms and directions it assumes in behavior.[2]

From this standpoint, many familiar terms in psychology appear to be various expressions of this postulate. Among these, the term "need" is especially conspicuous, for it turns out to be simply a regularly made psychological assumption—and any other usage is a pseudo-physiological one. A state of need (or energy-build-up or shift of flow) is always present in all behavior, and constitutes, really, a postulate for psychology, not an explanation (cf. Olds, 57).

Drive-modification theory, then, simply assumes that the organism is always active, always energized. In no way is this theory really concerned with energy itself, but only with the expenditure of energy and with the control of this release.

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[2] Discussion with Professor Herbert Feigl, however, leads me to point out that the division of labor here proposed may well be only temporary; it goes without saying that physiological and psychological principles must be mutually compatible.

D. Drives

In the theory considered here, the term "drive" refers only to the original innate modes whereby energy is expressed. That is, drives represent the original form of motivation, defined as the biological systems existing at birth, by which energy is expended. Five classes of drives may be recognized, as follows:

1. Life-maintaining systems, or visceral drives, in which the predominant state arises from conditions affecting the smooth muscles.
2. Life-producing systems, or activity drives, in which the predominant state arises from conditions affecting the striped muscles and some, at least, of the endocrine glands.
3. Experience-producing systems, or sensory drives, in which the predominant state arises from conditions affecting the special senses and their neural connections.
4. Experience-utilizing systems, or neural drives, in which the predominant state arises from conditions affecting the central nervous system, especially the association areas of the brain.
5. Experience-adjusting systems, or emergency drives, in which the predominant state arises from conditions of sudden excitation or depression of the autonomic nervous system.

No one of these systems is of any more basic significance than any other —all are primary processes and all operate in all behavioral acts. All are innate modes of expression or release of energy. Once energy gets into the system (of course, this condition arises as soon as cell-growth is initiated following conception), then drives have their potential appearance. Behavior may be instigated by changes in energy expenditure in any of these systems. But it is not the fact that one system rather than another is activated, which characterizes a drive-state, but rather that one sysetem is relatively more active than another. This temporary organization of drives is called, here, "predominance." It means an organization of drives such that one kind of system is at that time utilizing relatively more of the available energy and, as a consequence, is instigating a significantly larger segment of behavior.[3]

Note, incidentally, that all drives are here regarded as having a positive, or energy-utilizing character, none as negative. Efforts to divide impulses into positive and negative classes, such as appetites and aversions, are misleading, for the description of motivation in negative terms only refers to the outcome of a behavior sequence, not to the conditions which instigate that behavior.

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[3] The term "predominance" is chosen, here, rather than "dominance" (cf. Murphy, 52) for several reasons, including the fact that the latter word has specialized meaning in Murray's list of motives (55) and in genetics.

The five classes of drives listed here correspond in large measure to those proposed by Murphy (52), but with the addition of a fifth ("neural") not separately proposed by him. Dissatisfaction with the conventional limitation of drives to the visceral category is evinced, also, in Woodworth's suggestion that a perceptual (here sensory) drive ought to be recognized (73), (L. Murphy's reports on the play of preschool children also supply strong evidence for sensory drives, 54), in Hebb's notion of a cortical (here neural) drive (28), as well as in various other places.

The principle of "predominance" bears affinities to the "activation theory" of Duffy (18), and Lindsley (42), except that a continuum of activation, or instigation, is postulated for all five classes of drives, not alone for emergency (or emotional) states. In their terms, the upper ranges of the continuum of emotional activation would correspond to the predominance of autonomic states; similarly, the upper ranges of the continuum of visceral activation would correspond to the predominance of internal organic states, and so on. In each case, what we call the drive-state, in usual terminology, represents an organization of the various components such that energy-utilization is most active in one of the five kinds of system. Finally, it seems clear that the optimum organization is at times short of total predominance. That is, analogously to the contention of the activation theory of emotion, the highest levels of activation are disruptive of behavior for all classes of drives. The organism may be overwhelmed, so to speak, not only by extreme emotional activation, but by too intense visual or auditory or vestibular stimulation, by too powerful a predominance of muscular response, by an overrunning of cortical activity, or by extremes of visceral activity.

It is quite possible that many pathological states could be interpreted as illustrating the effects of extreme activation of cortical processes. The traumatic effects of blinding light or very intense sound may also be mentioned.

E. MOTIVES

In agreement with the functional autonomy theory and with the explicit statement by McClelland *et al.* (44, p. 28), drive modification theory maintains that innate drives give way to adult motives as a function of learning. However, there are several points that require explanation. In the first place, no absolute necessity exists that all adult motives are learned—i.e., drives *may be* and *usually are* changed by learning, but it depends upon the character and depth of the learning process. Furthermore, learning may have quite different consequences in different individuals or cultures with respect to the various classes of drives.

An important question pertains to the kind of effect which learning may have. It is possible to discern three of these possible resultants, as follows:

(*a*) Learning may lead to the acquisition of new tendencies which supplant the original drive. This position corresponds to the functional autonomy theory, and implies that learning may result in entirely new motivational systems.

(*b*) Learning may bring about the establishment of differentiated tendencies which act as instrumental mechanisms in the interest, often indirect (or requiring only rare reinforcement), of more basic innate drives. This position corresponds with a literal statement of drive-reduction theory. Here, presumably, both primary and secondary tendencies continue to determine behavior.

(*c*) Learning may eventuate not only in the establishment of differentiated mechanisms for drive-satisfaction, but also in pervasive changes in the original drives themselves, so that they no longer exist in their innate form.

Drive-modification theory adopts the third of these interpretations as a working hypothesis. Although, admittedly, very little satisfactory experimental evidence exists as yet with reference to it, there are many other sorts of evidence which, taken in toto, make the hypothesis seem very sound. It must be assigned to some future venture to do more than to state the sorts of evidence and to provide a few illustrative references. Foregoing the temptation to cite, here, very striking manifestations of drive-modification which could be culled from clinical literature, at least five lines of support for the hypothesis may be mentioned.

1. *Infant Development*

The study of changes taking place shortly after birth has led to a general principle that learning processes play a basic role in the modification and organization of the "raw materials" of drives in a fashion which could not possibly be explained as the gradual working out of predetermined hereditary tendencies. As an example, consider the differences in infants subjected to a strict four-hour feeding schedule as compared with a three-hour schedule (47). The internal economy of the infant evidently underwent marked change, in accordance with the external demands placed upon it, since each group of *S*s displayed a progressive adaptation to the imposed schedule and appeared to be upset by departures from it.

Consider, also, studies which reveal pronounced effects of environmental deprivation (6). Such studies have, perhaps, not been directed cleanly towards an understanding of changes in drives—and some psychologists appear to question the permanence of other effects measured (14). But there is at least the probability that enduring changes in drives may be found, were appropriate observation to be made.

As a further instance of the point, attempts to trace the early stages of emotional development lead to the supposition that innate tendencies merely provide the basic ingredients of later emotional patterns (7). Thus Bayley's results (3) strongly suggest that patterns of crying are significantly affected by the situations to which an infant is exposed (i.e., there is an experimental factor over and beyond maturational changes).

2. *Effects of Early Experience*

Hilgard (29) has drawn together in concise fashion scattered evidence for the progressive establishment of motivational patterns which probably differ in ultimate organization from the initial drives involved. As one example, there is considerable evidence that prolonged and severe independence training in infancy may be responsible for the acquisition of strong achievement motives (see McClelland's review of this evidence, 45; see, also, Douvan, 16).

3. *Sleep*

An explicit demonstration that even in the adult the basic organization of drives may be modified is the well-known experiment conducted in Mammoth Cave by Kleitman (37). During the period when two subjects lived under conditions of a six-day week, the new regime brought about in one of the two subjects a striking change in the internal processes associated with sleep and waking; it is probably not surprising that he was a young man, in comparison with the older subject, who did not show these changes. This experiment, of course, did not prove that a permanent modification occurred, for, presumably, life in Mammoth Cave was abandoned after the experiment. The study does, however, demonstrate a change in internal organization as a function of environmental conditions—an essential point for drive-modification theory.

4. *"Critical Situations"*

In this sort of evidence, the point concerns the degree to which so-called "secondary" or instrumental drives may be extinguished, with a revision to "primary drives." That is, the issue is whether or not functional modifications of primary drives ("secondary drives") may determine behavior in ways which cannot be accounted for on the supposition that primary drives continue to act as the significant determinants of behavior.

From this point-of-view, one is tempted to cite a wide variety of non-experimental cases, involving suicide, religious fanaticism and martyrdom, miserliness, self-starvation, and so on. Fortunately, there are at least some data of a more empirical sort. In particular, in the Minnesota study of semi-starvation (35), although some subjects appeared to revert to a state of the predominance of what are usually called primary drives, others never did so, but continued to act in ways suggesting permanence of what are often called secondary drives.

5. *Cross-Cultural Studies*

Probably the most striking evidence for drive-modification is found in the comparison of different societies in terms of motivation patterns (see Klineberg's lucid review of such material, 38; as well as the profound analyses of Kardiner, 33, 34). A case in point is the striking differences between the Alorese (17, 34) and the Balinese (4, 49). The former are characterized as having very strong and continual aggressive tendencies to such an extent, as Kardiner puts it, that "status in this situation can be maintained only by a process of continuous validation through the exercise of aggression in the form of ingenuity and chicanery" (34, p. 162). On the other hand, in Bali, the absence of overt aggression in all its forms (stylized and fantasy aggression, however, is frequent) is quite remarkable.

The work by such investigators as Whiting and Child (70) is also pertinent. The correlation between childhood training practices and adult attitudes strongly supports the essential thesis of drive-modification.

All of these kinds of evidence lend forceful support to the thesis that drives may undergo progressive modification in the most fundamental sense. Let us next consider the general consequences of this process.

F. TWO EFFECTS OF LEARNING

Implicit in the foregoing discussion is the notion that there are two kinds of drive-modification, one a direct change in drives, where the continuity is either unassailable or readily inferred, and the other a departure from drives where the continuity can only be seen from a thorough longitudinal study of the same persons. All these changes require a new term, in order that confusion in meaning be avoided. We shall call all instigating (as opposed to regulating) systems derived from drives "motives." Thus, the first resultant of learning, involving a progressive elaboration, refinement, differentiation, and organization of the original drives themselves, may be viewed as the derivation of sensory, activity, cognitive, visceral, and emotional motives. The second resultant, having to do with the progressive acquisition of new patterns of response, leading, by a step-by-step process to new classes of instigation, not traceable to drives save through complete knowledge of the experiential chronology of a person, may be called "acquired" or "social" motives. Note, again, that we are here employing the term "motive" to refer to all basic modifications of drives, and hence intend not only to refer to "achievement," "affiliation," and so on as motives, but also "hunger," "fear," "aesthetic" impulses, "understanding," and the like. (The statement must be qualified to the extent that not all drives are of necessity modified directly; a case in point might be "respiration" which could properly be called a "drive," but is ordinarily not much subject to learning, save, perhaps, in mile-runners, singers, certain small boys, etc.).

All psychologists no doubt recognize the importance for an understanding of human behavior of both kinds of motives. The differences of interpretation arise, so far as the basic properties of motives go, in two respects. The first issue concerns whether one class of instigating systems is more basic or pervasive than another. Thus, one may choose to give a central position to any of the motives directly stemming from the five classes of drives. Of these, there appear to be no clearcut modern statements of the primacy of neural or sensory drives, and only incomplete formulations of activity drives (see Harlow, 27, whose writings may really be construed as an attempt to encourage adequate recognition of the significance of activity drives more than to assign them an exclusive role). Thus, major controversy appears to concern emotional[4] vs. visceral drives as the primary source of instigation. The reasons for these choices lie in the fact that it is easy to judge either life-maintaining or life-adjusting processes as absolutely essential to all behavior, and it is perhaps in this respect that the historical differences between hormic and hedonistic views emerge most clearly today. If one chooses visceral drives, he is convinced that the organism has to live in order to behave—but this represents a confusion between the energizing and instigating functions discussed earlier. It also ignores the fact that an organism could not survive without sensory, neural, and activity drives (and at least in the human species without acquired motives, so far as that goes). In short, visceral drives and motives constitute one component of motivation, and our principle of predominance states that it may be primary or not depending upon the organization of the motivational system.

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[4] Space limitations preclude analysis of this component of motivation. For our purposes, "emotional" and "affective" are treated as synonymous, but covering the entire range of feeling and emotion, in accordance with the activation principle.

In the case of emotional drives, the argument probably rests upon the search for some tangible mechanism whereby the behavior of an organism in discriminating among various consequences of an action can be accounted for. Generally pleasant feelings appear to have different consequences from generally unpleasant, or painful, feelings. At quite high levels of activation, such "positive" and "negative" reactions can be made to do yeoman service in explaining motivation. Drive-modification theory would in no sense diminish the importance of emotional processes, but would, as for other components, consider them to be at all times an aspect of the motive-organization (contributing distinctively to it) and at certain times to be predominant.

On the whole, therefore, there is no good reason to treat any component as more basic than any other. Although clearly concrete "needs" and "rewards" seem to mark visceral or emotional processes—i.e., it is less easy to imagine that instigation may arise merely as a function of conditions in the brain or in the sensory systems and to identify the changes in behavior which ensue—this is surely to a large degree illusory, depending more upon the historical roots of psychology in physiology, animal behavior, and Greek philosophy than in any genuine effort to investigate the motivational functions of other possible classes of drives.

The second issue concerns the extent to which acquired motives should be considered equivalent in instigating properties to the direct derivatives of drives or whether they are to be regarded in all cases merely as instrumental or regulating mechanisms, despite their complexity and remoteness from the original drives. The functional autonomy theory attempts to resolve this issue by placing all problems of motivation on a potentially instrumental basis, but this effort has failed mainly because it leads to an anarchical picture of motivation. One cannot arrive at a complete understanding of motivation any more by stressing regulative than by stressing instigating processes. The real problem is to explore the relations among them by explicitly formulating the level of inference involved and by finding techniques to improve our inferences.

Since this second issue pertains to a comparison of primary and secondary processes, we turn next to this matter.

G. Primary and Secondary Processes

It must be evident by now that drive-modification theory does not consider the frequently made distinction between primary and secondary drives as tenable. "Primary" in this context can mean two things, on the one hand, simply "first" or "earlier," and on the other hand, "basic" or "unmodified." The former meaning can be accepted as expressing one pole of the chronological problem of motivation. This meaning is contained in the just-made distinction between "drives" and "motives," for all "drives" are earlier properties of the organism. In this sense, no "acquired" or "social" motive is "primary," for all are derived from drives. But whether or not visceral, sensory, neural, activity, and emergency instigators are "secondary," depends upon the degree to which the corresponding drives have undergone modification. It appears very likely that in most adults this process under normal developmental conditions has been extensive, so that we are justified in shifting our general terminology to that of motives rather than of drives.

The second meaning relates to the organization of the total motivational state at any given time. The principle of predominance signifies that the "primary" instigation is a function of the system which at that time has captured and is utilizing relatively more energy than other systems—i.e., it is a matter of the system (or systems) operating at the highest level of activation. Thus, any motive may potentially be primary, with any combination of other motives as secondary. This is not to say that systematic study would not reveal that certain motives in a sample of persons would not very regularly be predominant, nor that for any single person a particular kind of motive would not be so frequently predominant as to be called primary for him. It is to say that primary depends upon the organization of the motive system, either at particular times or in the enduring motive-structure of a person.

It is certainly a moot point whether a valid argument, on the basis of any published research, can be made for the notion that the survival of adult human beings depends solely upon behavior instigated by visceral motives. Without such evidence, it is just as meaningful to say that a person's survival depends upon satisfaction of his achievement motive, or his aesthetic motives, and hence that he eats, drinks, and/or engages in coitus, in order to make achievement or sensory gratification possible, as to put it the other way around. In fact, I would venture to say that for a normal, healthy person it would be truer that visceral satisfactions are (or at least may be) instrumental to social motives. Actually, to argue this way precipitates us into the same pitfall that the predominance principle tries to avoid.

The study of motivation must free itself from an arbitrary distinction between primary and secondary motives, in favor of investigations of the entire motive organization. There is no necessary and inevitable pattern of instrumentality among human motives. Drive-modification theory would allow as much potential primacy to social motives as to any other class, depending upon what occurs in the development of the individual.

H. Regulative Processes

All of this is not to ignore the fact that the term "secondary" may be a meaningful way to speak about the mechanisms by which instigation is made to lead to definite sorts of overt behavior. That is, motives are never expressed or satisfied directly, but always through the agency of attitudes, which may be defined as learned regulators of motives, linking them with goal-objects and determining what an individual does following instigation. Attitudes regulate the direction, consistency, content, specificity, sequence, and so on, of responses.

We have expressed the viewpoint that adequate research in depth and breadth will reveal that social motives may, on occasion, be just as predominant as other motives. The sorts of empirical evidence most favored by academic psychologists are simply not available to compare social with visceral or other motives in this respect. However, there is much more experimental evidence for the distinction between instigating and regulating processes. Some selection of data, of course, must be made, and our choice here will be studies by Frenkel-Brunswik and Klein, and the proliferating program of investigation stimulated by McClelland.

The first example was reported some years ago by Frenkel-Brunswik (24) and deserves more attention than it appears to have been given. She sought to correlate measurements based on Murray's descriptions of acquired motives with various "manifestations" of them (corresponding closely to the present concept of attitudes). Several predictions that would follow from drive-modification theory were borne out. Thus, *S*s judged to be similar in "recognition" and "aggression" differed markedly in the modes of behavior by which these tendencies were expressed, and, conversely, *S*s inferred to be different in underlying motives might be similar in "manifestations."

The work by Klein (36) has been characterized by a penetrating awareness of the theoretical and methodological advantages to be gained from distinguishing regulative from instigating processes. His experimental work has put the distinction to good use. In experiments with thirst as a motivating condition, thirsty and sated groups were sub-divided on the basis of what we would call attitudinal differences, and the behavior of these sub-groups, despite the inferred similarity of motive-state, was strikingly different. Of the several experiments, one may be cited as an explicit illustration. Thirsty

*S*s identified as "constrictive" in control displayed tightly centralized patterns of fixations in a tachistoscopic recognition test, whereas thirsty "flexible-controlled" *S*s showed a very loose, widely-dispersed fixation-pattern. In addition, the latter were more accurate on recognition of the test items, having an advantage in judging the peripheral stimuli.

Finally, we come to the highly provocative research by McClelland and his associates, who have done so much to advance the experimental study of motivation.

The initial phases of McClelland's work are too well-known to require extended explication. He proposed that motives may effectively be assessed indirectly through fantasy productions, as reflected in thematic apperception stories. Scoring systems have been worked out to provide indexes to the strength of tendencies aroused in some appropriate manner. In *The Achievement Motive* are presented a variety of studies with this technique and a great many more have been published since then. The question which concerns us, however, has to do with what motivational functions are actually to be inferred from the use of these arousal- and fantasy-interpretation experiments. More particularly, can we see in them any evidence for the distinction between motives and attitudes? Partly, the question concerns validation of the obtained measures, a point to which we shall return later.

It is necessary to understand that two quite contrasting techniques have been used in these affective-arousal experiments, and that they yield different conceptions of the motivational processes involved. In one method—that developed in *The Achievement Motive*—all subjects are treated as if they were much alike, motivationally speaking, in advance of the experiment, the conditions of which are conceived to bring about differences in inferred motivation. Thus, after assignment to experimental and control groups—or to non-aroused and aroused, or relaxed, task-oriented, and achievement-oriented groups—*S*s are exposed to the experimental conditions. Following performance they are administered some version of the TAT and their stories are scored to see if the content differs in a fashion which can be interpreted as a consequence of the difference in experimental conditions. Such experiments have been carried out, not only for achievement, but also for other variables, including affiliation (59), sex (11), and power (64). In most of these experiments the stories of the presumably aroused groups are found to contain striking signs of the aroused factor. The point is, however, that from such results, we really do not know whether the differences are a function of the bona fide arousal of motives, in the present sense, or of some regulative or attitudinal process stirred up by the specific situation. In the experiment by Veroff (64), for instance, the "aroused" *S*s were student candidates awaiting the results of an election; it might be expected that their TAT stories would reflect this situation as much or more than whatever impulse led them to compete for office in the first place. The point is nicely illuminated by experiments which failed to yield the hypothesized results, outcomes which are puzzling if we assume motivational instigation, but much clearer if we assume that the arousal conditions brought about additional differences in the *S*s.

For example, in an investigation by Clark on sex (11), marked discrepancies among three experiments were found in the incidence of sexual imagery. In one experiment, a group of *S*s witnessed achromatic slides of nude females whereas a control group looked at neutral pictures. In a second experiment, matters were arranged to stimulate (in a socialized manner) the experimental *S*s by an attractive, clothed female *E*, with a control exposed to a male *E*. In both of these experiments, TAT stories revealed significantly *less* sexual imagery in the "aroused" *S*s than in the control *S*s. In a third experiment, slides of nude females (in color this time) were shown at a beer party, on the theory that inhibition would be lessened in this atmosphere. In fact, the experimental *S*s now did produce more sexual imagery than the control *S*s. This difference is interpreted as a function of the relation between guilt and manifest expression. In the first two experiments, sexual arousal was accompanied by guilt, which inhibited sexual expression, thus reducing guilt. With the alcoholic condition, "the guilt over sexual arousal is reduced enough to permit the expression of sex with a resulting increase in expressed guilt."

The important thing for us is not so much Clark's explanation as it is the fact that the TAT scores presented such contrasting results under different arousal conditions. It is evident that the regulation of the motive (whatever it was) differed markedly, depending upon the experimental conditions.

Beyond this, however, such experiments actually tell us very little about motives, in the sense meant here. We do not really know, in short, to what the TAT measures are indexes, for they may merely reflect the immediate situation (an attitudinal factor) rather than a deeper and more significant property of the individual. Thus, all *S*s are treated as if they were equally arousable—equally marked at the outset by sex motives.

A closer approach to the problem of motives is represented by the second technique developed in the affective-arousal approach. Here, an attempt is made by means of a pre-test to assess the motivational strength of the *S*s (2, 13, 21, 22, 23, 26, 41, 71). TAT stories may first be obtained under *neutral* conditions or some other test designed to reflect motives may be employed.[5] On this basis, *S*s are divided into groups "high" and "low" on the selected variables, and *both* groups are subdivided into experimental and control groups (e.g., "non-aroused" vs. "aroused") and then exposed to the requisite conditions of the experiment. Following this, *S*s are retested with the same instrument.

Nearly all such experiments bring out interesting facts about the differential effects of the arousal situation. What happens, essentially, is that persons who, according to the pre-test, differ in motivational properties, may be affected quite similarly by the experimental conditions—or, under more complex conditions, the "highs" are more aroused by one sort of situation, whereas the "lows" are more aroused in another. These results are what the drive-modification theory would predict. An example is an experiment on affiliation motivation by French and Chadwick (22). Here, it was found that those high on affiliation were more aroused, on the average, by a situation designed to instigate affiliation, than were the lows, but in at least one sub-group (the unpopular *S*s) those low in affiliation showed a large increase in affiliation score following arousal. In another experiment by French (21), on achievement, initially high and low *S*s were compared under relaxed, task, and extrinsic orientations. For the first two conditions, both post-test and performance measures were higher for the *S*s high in achievement, but under the third condition, this result was reversed. Still another instance is found in an experiment on achievement motivation by Lazarus, Baker, Broverman, and Mayer (41). Although the "highs" and the "lows" differed markedly under "practice" conditions, this difference was very much less under "test" conditions (see, also, Wendt, 68).

The distinction between attitudes and motives is also demonstrated in an experiment by Birney (5). Groups divided into high and low achievement *S*s on the basis of the TAT performed similarly on a series of simple tasks when the *E* was a faculty member, but much differently with a student *E* [agreeing in this latter case with earlier results obtained by Lowell (43)]. Space precludes citing more experiments, but in general the conclusion is well-stated by Vogel, Baker, and Lazarus (65) on the basis of their results, that "whether a need will be expressed in fantasy, in behavior, or not at all, is a function of regulative processes in the personality."

In general, then, a meaningful distinction can be drawn between instigating and regulating processes, with the latter demonstrated especially clearly in experiments where factors in the situation itself are reflected in differences in the manifestation of a motive. Similar results, at a typically more superficial level have been reported innumerable times under the heading of "set," "Einstellung," "determining tendency," and the like. The inferred processes may be thought of, in the present view, as attitudinal in character, but perhaps more specific than the classes of attitude properly to be regarded as relatively permanent and important regulators of motives. Thus, a "set" might be differentiated from an "attitude," as a specific mechanism whereby an attitude is adjusted or focussed as a function of the temporary conditions of the situation.

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[5] A very promising instrument for this purpose is the Edwards Personal Preference Schedule (19). All such tests still require much in the way of validation, however, as pointed out below.

I. VALIDATION

The final question to which this discussion leads concerns what is being measured when one employs the tests presumed to provide information about motivational processes. That is, one would like to know whether the TAT, or French's "Test of Insight" (21), or the Edwards Personal Preference Schedule, supplies information about attitudes or motives. This problem comes down to the validation of the tests. That a real problem arises, here, is demonstrated by Child, Frank, and Storm (10), who found only very slight relations between TAT scores and self-ratings, or between either and items on a childhood background questionnaire. (See, also, de Charms, *et al.*, 12.)

What criteria is one to use? McClelland, Atkinson, Clark, and Lowell (44) suggest that a person's achievement motivation may be high "because of his 'internal' cues, because of cues in an immediately preceding experience, or because of cues in the pictures" (p. 207). They appear to feel that measures reflecting any of these cues may well provide equally valid information, but the second and third kinds are said to be preferable. In fact, internal cues are positively rejected on the grounds that a person whose achievement impulses are thus aroused is probably neurotic (p. 209). This point-of-view is at odds with the drive-modification theory of motivation, which maintains that the internal cues of greatest significance in personality determine the most typical pattern of motivational predominance. In all likelihood, changes brought about by immediate experience or pictures may reflect attitudes (or sets), rather than motives. On the other hand, we must not rule out the possibility that these latter two kinds of response may prove to be indexes to patterns of predominance. And it is precisely here that validation studies are urgently needed, lest we assume that an index refers to motives, when it really refers to attitudes.

It is true that a vast amount of interesting experimental work can be done by manipulating attitudinal and set variables. It is even possible that eventually the sorts of specific and limited predictions we might be able to make after the painstaking accumulation of such data would add up to a general knowledge about motivational variables.

But a much more sensible procedure, on the whole, would be to tackle the validation problem directly. [This need is fully recognized by McClelland (45) who advocates cross-cultural studies.] According to the drive-modification theory, the only adequate criteria of motives (as distinct from attitudes) are long-continuing, general, and important characteristics of behavior. This means that an achievement motive, for example, represents an instigating system which has had a significant history of predominance in a person's life; a person "high in achievement" is someone who has displayed over a long period of time a tendency to seek success, mastery, overcoming of difficulties, and so on [see Murray (55) for other signs of achievement]. It would require an assessment of the life-pattern of a person to yield meaningful evidence concerning the strength, importance, and frequency of achievement as a motive.

This, of course, represents a complex and difficult task, and even signifies that college students can hardly be expected to be an adequate validation population, for they have probably not yet reached a point in development where their motives can be dependably differentiated. Rather, intensive study of adults would be required. This task has seldom been attempted, but the program begun by Murray is one example. The follow-up studies of three cases, reported by White (69) illustrate what could be learned.

If one had case-histories of normal adults, appropriate judging techniques could be applied to assess the relative importance and strength of motives. Criterion groups could then be established on the basis of this assessment, so that we would have high and low achievement, high and low affiliation, high and low nurturance, high and low esthetic, high and low activity, and so on, samples. We would then seek for measures which satisfactorily discriminate among these groups. It is possible that TAT indexes, or the Edwards Personal Preference Schedule, or other tests would do the job. But at present we really do not know that they do.

J. SUMMARY

There are three principle kinds of problems in the psychology of human motivation, having to do with (*a*) structure, (*b*) function, and (*c*) chronology. The history of psychology has seen four types of theories which attempt to cope with these problems, namely, *instinct theory*, *functional*

autonomy theory, *drive-reduction theory*, and *drive-modification theory*. This paper attempts to develop the character of this fourth theory, together with the implications which follow from it. Major points concern energizing, instigating, and regulating processes. A chronological distinction is drawn between drives (innate biological systems) and motives (modifications of these systems as a consequence of learning). A further distinction is made between motives (instigating processes) and attitudes (regulative processes). Matters of motivational organization are dealt with in terms of the principles of activation and predominance. The need for validation research is pointed out.

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SOCIAL SKILLS AND THEIR RELATIONSHIP TO SCHOLASTIC ACHIEVEMENT*

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Over the past four decades, a considerable amount of energy has been expended in an effort to describe and understand the concept of the intelligence of man. As illustrative, over the past 10 years, no less than 490 titles of articles dealing with intelligence have appeared in the *Psychological Abstracts*. A perusal of these abstracts reveals a wide range of variability in the approach to the study of the concept of intelligence. One of the most common definitions that appears in the literature is that intelligence is the capacity to reorganize one's behavior pattern so as to act more effectively and more appropriately in novel situations. It has been further widely assumed that the ceiling of this capacity is set at conception and cannot be raised, but it may be lowered by environmental deprivation.

While this general and perhaps too simple definition appears to be accepted, the actual measurement of intelligence has long been a controversial problem. It appears to the authors that this controversy stems from the conventional means of measurement and the validation of the tests used. In specific terms, the test situations are set in a too limited frame of reference. The test items are set up to predict success in school—in academic learning—thus, these items have been properly validated from school grades in academic subjects. This construction and validation of test items has produced many contradictions in the prediction of behavior. The prediction contradictions in non-school activity might be expected, but errors in predicting of school achievement are not as easily accepted.

The findings of this study suggest that success in academic activity is based not only on certain abstract skills, but on skills and attitudes that are not tested by the conventional instruments.

Thorndike (9) early recognized that intelligence must not be limited to one type of behavior. Thus, he advised the use of three categories: "(*a*) Abstract or verbal intelligence, involving facility in use of symbols; (*b*) mechanical intelligence, involving facility in manipulation of objects; (*c*) social intelligence involving facility in dealing with human beings." In spite

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of this suggestion, most testing has continued to be based on items that appear to be components of skills needed to acquire school grades.

From a little different point of view, Wechsler (8) also emphasized the problem of too limited an interpretation of intelligence. ". . . . human intelligence, as the psychologist conceives it, can only deal with geometric and logical symbols. Now the remarkable thing is that while this is what we are saying in our tests of intelligence most of us don't believe it. What is more important, it isn't true! Our contemporary definitions of intelligence according to these is not only the ability to learn, to abstract, to profit from experience, but also to adjust and achieve." Wechsler points out, "crucial instances where individuals obtain identical *IQ* (say 65) but, in overall appraisal, must nevertheless be rated differently, say one as a defective and the other not defective."

It is interesting to note that in spite of the wide use of the conventional test, a search for new components of intelligence and new predictions of scholastic achievement have continued.

One of the interesting contributions in this direction are the studies of Gough (4), who by empirically selecting items from the MMPI and constructing similar ones, devised a scale of 52 personality inventory items which showed a significant correlation with conventional measures of intelligence. Mean validity coefficients of .51 and .43 for high school pupils and college students respectively can be obtained from the reported data.

Gough (5) devised another scale of 36 items, 16 chosen from the MMPI, which was used as a personality scale to predict scholastic achievement. Mean correlation between the devised personality scale and undergraduate grades of .38 is reported. A correlation of .26 between the personality scale and measures of intellect is cited as evidence that . . . "the Hr is a predictor of academic achievement and not simply an indirect and inefficient measure of intellect."

No statistical test of the significance of the difference between the means is offered thus only leaving Gough the hope that his scale measures a different potentiality than that measured by conventional intelligence tests.

It is the purpose of this presentation to report some findings which were discovered as a part of a larger study but are particularly germaine to the above problem of the understanding of intelligence. Over a period of years, Hall and Associates (1, 6, 7, 10) at the University of Nebraska have been studying social skills as an attribute of positive behavior. One of the techniques that has been developed in a peer rating device that, although originally constructed as a sociometric technique, has been developed into a rating scale. This scale has been designated *The Friendship Rating Scale.*

This scale has achieved its greatest effectiveness when used in a close knit group such as in a fraternity or sorority whose members live together by common choice. The administration of the scale is achieved by gaining the confidence and cooperation of the leadership of the group who in turn elicit cooperative participation on the part of the membership. Rating sessions are set up and each member rates every other member on nine items such as, "He greets other people when he meets them." The rating is made for each item on a five-point scale ranging from 1 (never) to 5 (almost always). The ratings for each house are reduced to a single number which makes it possible to place the individuals of the rated group in a rank order on the basis of the scale. A future report on a recent study will show that the ratings have a remarkably high degree of stability.

Over the past several years, it has been found in the studies at Nebraska (6) that the ratings on the Friendship Scale were the best predictors of counseling and teaching relationships. The objective of the larger study of which these findings is a part was to develop an interview which could be substituted as a shortcut for the rating scale measurement. The scale was administered to 20 houses, 11 fraternities and nine sororities. For the purpose of developing an interview instrument, 99 individuals were selected out of 1300 students who were members of the 20 houses. This group was chosen so that each house was represented (in proportion to the size of the house membership) in three equal groups—high, low, and middle. The high group designates students who had the highest friendship rating scores, the low group who had low scores, and the middle group, who had scores that were near the house mean. A.C.E. psychological examination scores were available for only 93 of the 99 students so that the number of cases was reduced to 93.

Correlations were run between the measurement variables. The study of Table 1 reveals that the correlation is higher between the rating scale and interview scores than the *L* and *Q* scores of the A.C.E. Of even greater importance is the fact that neither the rating scale or interview correlate significantly with the *L* or *Q* score.

It would appear that here is evidence of the measurement of a personality component of intelligence that is not related to the component that is commonly measured by the conventional intelligence test. Over a period of 10 years, most clinical experience with students that rate high on the rating scale has shown that these students do have considerable facility in dealing with human beings, but do not have necessarily a high *IQ* rating by tests.

To gain further insight as to the significance of this component, an analysis of Multiple Regression, using cumulative grade average as a criterion, was run. The hypothesis to be tested was to determine whether each of the prediction variables made a statistically significant contribution to the prediction of the grade average used as a criterion.

TABLE 1
ZERO ORDER CORRELATIONS BETWEEN MEASUREMENT VARIABLES
(N = 93)

	Symbols defined in column at left			
	x_1	x_2	x_3	x_4
y, Grade average	.51**	.51**	.36**	.22*
x_1, Friendship Rating Scale		.56**	.06	.10
x_2, Interview score			.18	.12
x_3, Ace L-score				.40**
x_4, Ace Q-score				

* For $N = 93$ pairs, a correlation of .204 was necessary for significance at the 5 per cent level (25, p. 424).

** For $N = 93$ pairs, a correlation of .266 was necessary for significance at the 1 per cent level (25, p. 424).

With the use of analysis of variance it was possible to determine the statistical significance of the difference between the multiple correlations of the four variable regressions obtained by eliminating each of the variables in turn. The F-values for the contribution of the A.C.E. Q-score to the prediction of grade average was found to be not significant. The loss due to the elimination of the rating scale score from the prediction variable changed the multiple correlation from an *R* of .65 to .57. The unique contribution of the rating scale score was .35 and significant at the 1 per cent level. The elimination of the interview score and then, in turn, the L-score changed the multiple correlations from .65 to .61 and .65 to .60 respectively, with unique contributions of 27 and 31, both of which were significant at the 1 per cent level.

It is recognized that this population, because of the selection method and because there is a variation in the ranges and means of the scores for different houses, the scores of the people included in the sample form more nearly a rectangular distribution of scores for the population from which the sample was selected. Thus, even more than in the typical study, restraint must be exercised in generalizing from the study. But without question it is true that a test not related to intelligence predicted somewhat better grade average than a conventional *IQ* test did for this population. Ninety-three cases in the study make too large an exception to prove the rule. Over the last few years several studies at Nebraska have revealed that skills in counseling and practice teaching are not related to intelligence as measured by the A.C.E. test. Furthermore, the rating scale did predict success in these activities at a significant level with correlations running at .40.

The above findings appear to warrant further careful study of this component of intelligence. They seem to suggest some justification for Thorndike's social intelligence involving the facility in dealing with people. This facility may be shown by students in their positive attitude toward their teachers. These students not only receive a favorable response from their teachers because of their skill in dealing with people but also because of a positive attitude generated toward learning opportunities provided by their teachers. Certainly much needs to be done to study and experiment further with a concept of intelligence that considers intelligence on a broader scale than that of the conventional test. One study has been completed that is soon to be written up which essentially supports these findings.

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CHANGES IN FREE CAGE BEHAVIOR OF RHESUS MONKEYS AS A FUNCTION OF DIFFERENTIAL DOSAGES OF NITROGEN MUSTARD*

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A. INTRODUCTION

The purpose of this study was to determine the effects, as manifested in changes in free cage behavior, of differential dosages of nitrogen mustard—methyl bias (2-chloroethyl) amine-hydrochloride—administered to rhesus monkeys. A second purpose was to secure information about the particular behavioral syndromes produced by mustard poisoning. Nitrogen mustard has been found to be radiomimetic in many of the physiological effects which accompany its use. Phillips (7) and DeLong (2) have separately reviewed the effects of nitrogen mustard compounds and discussed many of the similarities of the physiological effects of mustard poisoning with those of whole body ionizing radiations. The more important of these similarities are: (*a*) Under both agents hematopoietic cells are susceptible to destruction, and bone marrow and lymphoid organs react in a similar manner temporally and morphologically. Identity of action includes a similar, selective destruction of specific cell types, but begins sooner following mustard poisoning than after ionizing radiations. (*b*) Nitrogen mustard and ionizing radiations have a parallel action on intestinal mucosa. Atrophy of the testes and damage to spermatogenesis are also common results. (*c*) Both can destroy neoplastic tissues just short of permanent eradication. Tumors which are sensitive to both are largely of the lymphoma and neoplastic diseases of hematopoietic organs. (*d*) Both energizers appear to be relatively unique in their potent action against chromosomes and in their capacity to induce mutations. (A difference is found, however, in the statistical proportions of the various types

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These experiments were conducted at the Radiobiological Laboratory of the University of Texas and the United States Air Force, Austin, Texas; and supported with funds provided under contract AF 18(600)-165 and the School of Aviation Medicine, USAF.

of mutations.) (*e*) Both severely suppress antibody production. (*f*) Both inhibit cell division and induce fetal abnormalities in pregnant mammals. (*g*) Both agents produce alterations in citrate synthesis in certain tissues which are quantitatively similar. (*h*) Both agents enhance transaminase activity in the liver and both depress transaminase activities in the intestine.

Although there is striking similarity between the syndromes caused by these agents, Boyland (1) has pointed out,—"the use of the term, radiomimetic, is not meant to imply that the mechanisms of action are necessarily the same." The fact that reactive chemical agents can duplicate so many of the cytological actions of ionizing radiation is a puzzling phenomenon which is not as yet explained. A thorough exploration of the effects that both of these agents have on behavior could be a valuable technique for the discovery of physiological conditions underlying specific behavioral patterns. By studying a given observable reaction which may result from two distinct etiological factors, we may arrive at some understanding of the critical mechanisms involved.

Observation of free cage behavior was employed in order to identify variables which are appropriate for molecular analysis in the laboratory.

A large percentage of the psychological research done with animals has been concerned with sexual or food-object behavior, when in fact neither activity occupies more than a small fragment of the normal animal's spontaneous behavior. To ignore forms of activity which occupy the largest proportion of an animal's normal behavior misses the most sensitive areas of change. Nissen (6), in a discussion of the behavioral changes in chimpanzees following exposure to X-rays, remarked that, "Perhaps the most sensitive indicator of irradiation effect was 'cage behavior' in which there was no problem imposed by the experimenter, but the animal merely did, in its customary environment, what it pleased to do. . . ."

McDowell, Davis, and Steele (5) studied the effects of X-ray radiation on the behavior of monkeys who were systematically observed in their living cages. Their study showed that irradiation with X-rays brings about changes in free cage behavior which cannot be accounted for in terms of a decline in general activity level alone.

B. METHODS

1. *Subjects*

Twenty-four preadolescent male rhesus monkeys were used as *S*s. These animals were assigned in pairs to one of 12 cages.

Observations were made of *S*s in their living cages. All cages were located in two adjacent three-walled compartments in the colony area of the Radiobiological Laboratory, and were identical in size, construction, and equipment. Each cage was 5-ft. deep, 3-ft. wide, and 5-ft. high, and was of 2-in. wire mesh construction. The floors were concrete and the walls, which were also the rear boundaries of the cages, were made of concrete tile.

Subjects were arbitrarily assigned to one of four groups. They were placed two in a cage in a manner that minimized factors such as distractive stimuli, intra-compartmental positional features, and expected post-experimental social conditions. The arrangement provided that no cage would be either adjacent to or directly across from another cage containing *S*s in the same group, nor would more than one of the adjacent or directly opposite cages be occupied by animals of any other specific group. Each of the four arbitrary designations was then randomly assigned to one of the four experimental conditions.

Dosages were computed in milligrams of nitrogen mustard[2] per kilogram of body weight. Group I received 1.00 mg/kg; Group II (former controls), 1.75 mg/kg; Group III 2.00 mg/kg; and Group IV 3.00 mg/kg. The controls were injected with a saline solution.

2. *Procedure*

One pair of wooden blocks, two watering pans, and one permanent shelf were located in each cage for each observation. One block of the pair was 4-in. by 4-in. by 1½-in. with a 2-in. circular hole in the center. A 10-in. wooden cylinder 1½-in. in diameter was attached to the block by a 12-in. length of plumber's chain. One end of a 6-ft. length of plumber's chain was secured to the edge of the square block; the other end was equipped with a snap lock which was attached to the door of the cage. The blocks were placed near the middle of the cages at the beginning of each observation. The watering pans were circular, 8-in. in diameter and 2-in. deep, and placed on the floor side by side at the front of the cage permanently secured to a metal brace with stovepipe wire. The shelves were constructed of sheet metal, measured 15-in. deep and extended across the back of the cages.

Five hundred ten 5-minute observation periods were recorded for the study following a series of preliminary observations. The preliminary observations were made to acquaint the animals with the experimental situation. After habituation, control data were obtained on each animal by means of one observation per animal per day for five days. Post-treatment observations were begun approximately three hours after dosage, and were continued for 14 consecutive days, or until the animal's death when it occurred during the two weeks.

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[2] Methyl bia (2-chloroethyl) amine-hydrochloride.

3. *Selection of Categories of Behavior*

An attempt was made to obtain a sample of behavior as representative as possible of the total free-cage behavior patterns of rhesus monkeys; therefore, the activities subsumed under each category were of a gross nature. The eight categories which were arbitrarily selected and arbitrarily, though rigorously defined, were as follows:

a. Shifts in location. An act of locomotion with a definite point of origin and a definite terminus, excluding speed and distance factors.

b. Visual activity. A unitary, attention-directed, visual event (including the characteristic visual survey) which was not an integral part of other behavior (such as self-grooming).

c. Self-grooming. Any activity, or combination of activities, directed by an animal to his own body, including oral, manual, and visual attention. Also, the utilization of environmental features for self-grooming purposes such as rubbing, scratching, cleaning the body, etc. Unit was terminated by appearance of any behavior not descriptively appropriate.

d. Cage manipulation. Any oral or manual manipulation of the inanimate environment except the wooden block. Unit may include the use of different body parts on various objects. Terminated at instant no cage manipulation discernible.

e. Block manipulation. Any oral or manual attention-directed manipulation of any part of the wooden or chain portions of the block complex. Shifts of location, visual attention to non-block stimuli, or momentary self-grooming events may appear during a block manipulation unit as long as some degree of attention to the block complex is continuous; for example, chewing block while visually regarded threatening approach of cage mate.

f. Resting. Used when no overt activity was evident, and with minimal energy required to maintain position such as resting, embrace, deep slump, lying down, complete sag against cage part, etc. Any movement, other than breathing, or any visual attention served to terminate the unit.

g. Grooming. Comparable to *Self-grooming* except that it is other animal oriented, rather than self oriented. Hostile behavior, mounting and thrusting, play activity, and fortuitous contract excluded.

h. Aggression. Any hostile attack or gesture directed by an animal toward his cage mate, animals other than cage mates, or the observer.

4. *Collection, Treatment, and Analysis of Data*

The experimenter recorded the results of his observations by depressing microswitches. Three switches, activated singly or in combination, were connected to a kymograph and were used to record four categories. The remaining four switches activated electrically-triggered mechanical counters. Three of these also activated time clocks. Frequently data were obtained for all eight categories and duration data for seven. The kymograph was a Brown Electronic Potentiometer. The recording paper was drawn past a recording ink-supplied stylus onto a drive cylinder at a rate of 1.7 inches per minute. Duration and frequency data for four categories (resting, grooming, self-grooming, and visual activity) were recorded graphically on the kymograph. The amount of horizontal displacement of the stylus from the baseline position provided information both as to the type of activity and the frequency of its occurrence. The vertical distance covered by each horizontal displacement provided a quantitative measure of unit duration. Duration and frequency data for three other categories (cage manipulation, block manipulation, and aggression) and frequency data for shifts in location, were taken directly from individual time clocks and mechanical counters and recorded on the corresponding kymograph sheet.

The data were tabulated and the totals in each category per animal per observation were recorded. These data were reduced to four scores per category per animal by summing scores into periods of five consecutive days. These summed scores were converted to ratios of total activity so that quantitative changes due only to reduction in general activity level would not influence the results. The first 5-day score for each animal consisted solely of control data, and the remaining three scores were posttreatment data. Since several animals died during the experiment, those 5-day periods which were represented by less than three scores were omitted from the analysis.

TABLE 1
RELIABILITY COEFFICIENTS
(Computed by odd-even method)

Category	Coefficient of stability
1. Shifts in location	.79**
2. Visual activity	.78**
3. Self-grooming	.66**
4. Cage manipulation	.69**
5. Block manipulation	.77**
6. Resting	.64**

** Denotes significance at or beyond the 1 per cent confidence level.

Since the nature of the data did not meet the assumptions of the analysis of variance, the White rank-order test (3) and the Kruskal-Wallis Test (8) (a nonparametric one-way analysis of variance) were used. Results were considered significant if a confidence level of 5.0 per cent or beyond was obtained. Coefficients of stability were computed for each category using the odd-even method (see Table 1). These reliability coefficients ranged from 0.64 to 0.79 and all were significant beyond the 1.0 per cent level of confidence.

C. RESULTS

The results are presented as morbidity and behavior data.

1. *Morbidity Data*

The effects of giving nitrogen mustard to rhesus monkeys intravenously are shown in Table 2. The 50.0 per cent kill at 2.0 mg/kg dosage is in accord with the LD-50-30 reported in pharmacological literature (7), and the morbidity at the other doses indicates the steepness of the dose-death curve. One striking feature of these results is the rapidity of death following dosage, contrasted with the much longer premorbidity period in midlethal dosage with whole-body ionizing radiations. The small number of *S*s in each group make further comments regarding the lethal dose curve impracticable.

TABLE 2
THE EFFECT OF NITROGEN MUSTARD POISONING ON SURVIVAL OF RHESUS MONKEYS

Treatment condition	Number survived	Number died	Interval between dose and death (in days after treatment)
Saline injected	6	0	
Group I (1 mg/kg HN-2)	6	0	
Group II (1.75 mg/kg HN-2)	4	2	4½ and 7½ days
Group III (2.00 mg/kg HN-2)	3	3	6½, 8½, and 7½ days
Group IV (3.00 mg/kg HN-2)	1	5	5½, 6½, 6½, 6½, and 7½ days

2. *Behavior Data*

The results pertaining to behavior were organized according to the eight categories studied. The ordinate of each figure indicated the ratio between the amount of a particular activity and the total activity occurring in all categories. Five of the six figures presented contain an *A* and a *B* part. The *A* part presents the relative number of times a particular behavior occurs (frequency), and the *B* part of the figure indicates the relative total time in seconds that *S*s engaged in this activity (duration). The abscissa of each figure is divided into four parts, each part representing a 5-day period of the study. The first 5-day period occurred prior to treatment and the remaining three periods occurred after dosage. The statistical comparisons which relate to each subfigure are concerned with tests between the performance of *S*s in the particular treatment groups following administration of nitrogen mustard and between *S*s' performance in each 5-day period of the experiment.

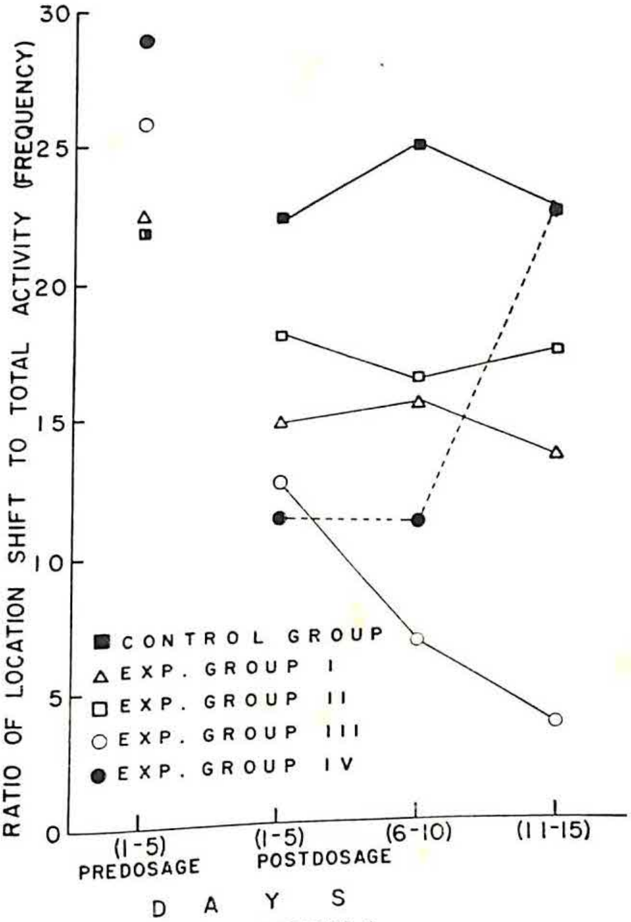

FIGURE 1

Relative Number of Locaton Shifts by Nitrogen Mustard Treated and Non-Treated Monkeys Before and After the Period of Treatment

a. Shifts in location. Figure 1 presents the ratio between the number of shifts in location to the total number of recorded activities of *S*s in the control and four treatment groups. The *S*s in the control group maintained a relatively constant proportion of location shifts throughout the pre- and three posttreatment periods, but *S*s in each of the experimental groups shifted location significantly less in three posttreatment periods than in the pretreatment period. Also, the relative number of location shifts registered by *S*s in each of the treatment groups during each posttreatment period is significantly less than the number registered by *S*s in the control group during comparable periods. Intergroup comparisons were made. Group III differed from both Group I and Group II at the 1.0 per cent confidence level, and Group II differed from Group IV at the 5.0 per cent confidence level. Since response duration was not recorded for this category of behavior only the frequency data were considered.

b. Visual activity. Figure 2-A shows the relative frequency and Figure 2-B indicates the relative cumulative duration of *S*s' visual activities. No significant differences were found between performances of *S*s in the five groups or between the performance of a particular group pre- and posttreatment. However, all *S*s showed an over-all decline in the relative frequency and relative duration of visual activities.

In Figure 2-A the relative frequency of visual activity of all *S*s during the pretreatment period is significantly lower, at the 5.0 per cent confidence level, than during the first posttreatment period. The decline between the first and second 5-day posttreatment period is significant at the 1.0 per cent confidence level, and the differences in performance of all *S*s between the first and last 5-day posttreatment period is significant at the 5.0 per cent confidence level. None of the remaining comparisons between the various periods of observation are significant. In Figure 2-B, *S*s have visual activities of significantly shorter duration in the second and third posttreatment periods than in either the pretreatment period, or the first posttreatment period ($p = .01$ and .05 respectively in both sets of comparisons).

c. Self-grooming. The relative frequency and relative cumulative duration of *S*s' self-grooming responses are shown in Figures 3-A and 3-B respectively. The prominent feature of both sets of results is the divergence of the performance of *S*s in the four treatment groups from the performance of *S*s in the control group. In Figure 3-A (frequency of self-grooming) the performance of *S*s in each treatment group is significantly different from the performance of *S*s in the control group ($p = .01$ in comparisons between the control group and Group I, II, and III and .05 between the control group and Group IV). None of the intergroup comparisons for *S*s in the experimental groups are significant. These trends are not evident until the second posttreatment period and analysis bears this out. There is significantly more self-grooming in the second posttreatment period than in either the pretreatment period, $p = .05$, or the first posttreatment period, $p = .01$. There is also significantly more self-grooming in the third posttreatment period than in the pretreatment period, $p = .01$. The durations of self-grooming are longer in the third posttreatment period than in either the control pretreatment period, $p = .01$, or in the first posttreatment period. None of the other comparisons between groups of *S*s or between periods of the experiment are significant for either frequency or duration scores of self-grooming.

d. Cage manipulation. Figures 4-A and 4-B show the pretreatment and posttreatment functions of cage manipulation as measured by frequency and duration respectively. Unlike those categories which show a consistent reaction to the experimental variable, cage manipulation data during the posttreatment periods show a diphasic pattern. Both significant increments and significant decrements are found in the performance of *S*s in the experimental groups when compared with the performance of *S*s in the control group.

Differences between *S*s receiving the lowest dose, represented by triangles in the figures, and control group *S*s are slight and not significant. The *S*s receiving 1.75 mg/kg doses, indicated by open squares, show a tendency to manipulate the cage more often and for longer duration throughout the posttreatment period. The *S*s in this group manipulate the cage more frequently than *S*s in Groups I, II, and III during the posttreatment periods, $p = .05$, .01, and .01 respectively, and for *longer* durations than *S*s in the control group and each one of the experimental groups, $p = .05$, .05, .01, and .01. At a slightly higher dose, 2.0 mg/kg, *S*s inhibit cage manipulation. These *S*s manipulate the cage *less* during the posttreatment period than do *S*s in the control group and treatment Group II ($p = .05$ and .01 respectively). The durations of manipulations by these *S*s are *shorter* than manipulations by *S*s in the control group and treatment Groups I and II ($p = .01$, .05, and .01). The performance of *S*s in the group given the highest dose is similar to that of *S*s given 2.0 mg/kg nitrogen mustard and tests of differences are significant at the same levels except that the frequency of cage manipulations by *S*s in Group IV are less than the manipulations of *S*s in Group I, $p = .05$, as well as *S*s in the control group and Group II. Because of the diphasic dose response of cage manipulation only one of the 12 comparisons between the performance of *S*s in the various periods of posttreatment observation are significant; the treatment groups *S*s manipulated their cages more often during the last posttreatment period than during the intermediate period, $p = .05$.

e. Block manipulation. Extreme divergence, both in frequency (see

Figure 5-A) and duration (see Figure 5-B) of block manipulation, appeared during the posttreatment period between the performance of *S*s in each of the treatment groups and the performance of *S*s in the control group. Each

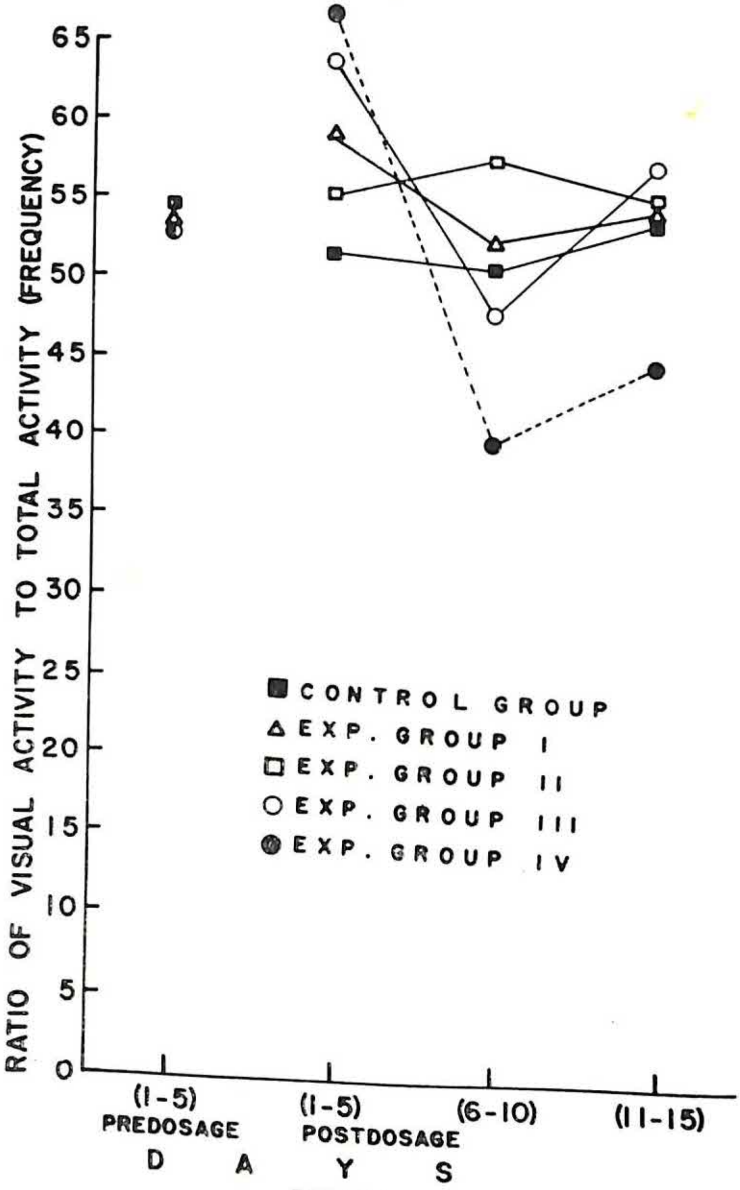

FIGURE 2-A
RELATIVE *Frequency* OF VISUAL ACTIVITIES BY NITROGEN MUSTARD TREATED AND NON-TREATED MONKEYS BEFORE AND AFTER THE PERIOD OF TREATMENT

of these differences is significant far beyond the 1.0 per cent confidence level. Also, *S*s in Group II manipulate significantly more often than *S*s in Group IV.

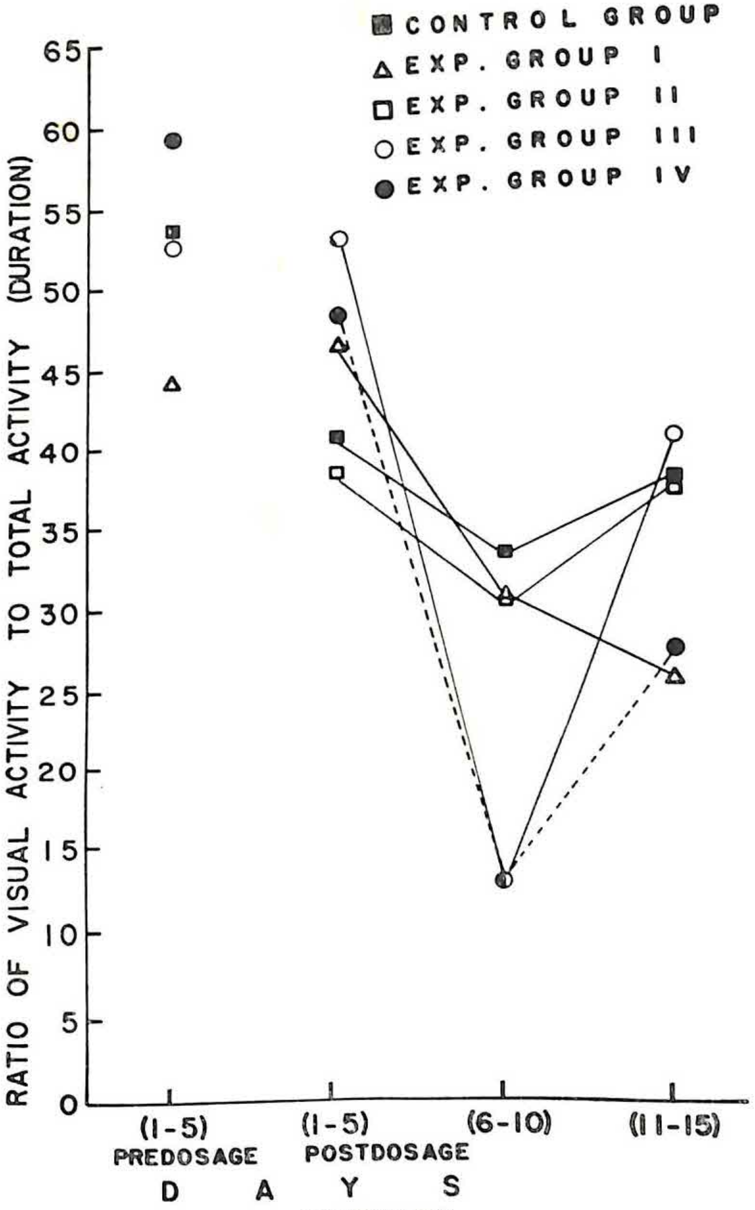

FIGURE 2-B
Relative *Duration* of Visual Activities by Nitrogen Mustard Treated and Non-Treated Monkeys Before and After the Period of Treatment

In general *S*s in the experimental group manipulated the block less and for shorter times during the posttreatment period than in the pretreatment period. This difference is significant in all but one comparison—between the predosage performance and performance in the last posttreatment period measured as frequency.

f. Resting. Subjects in each of the treatment groups rest more frequently,

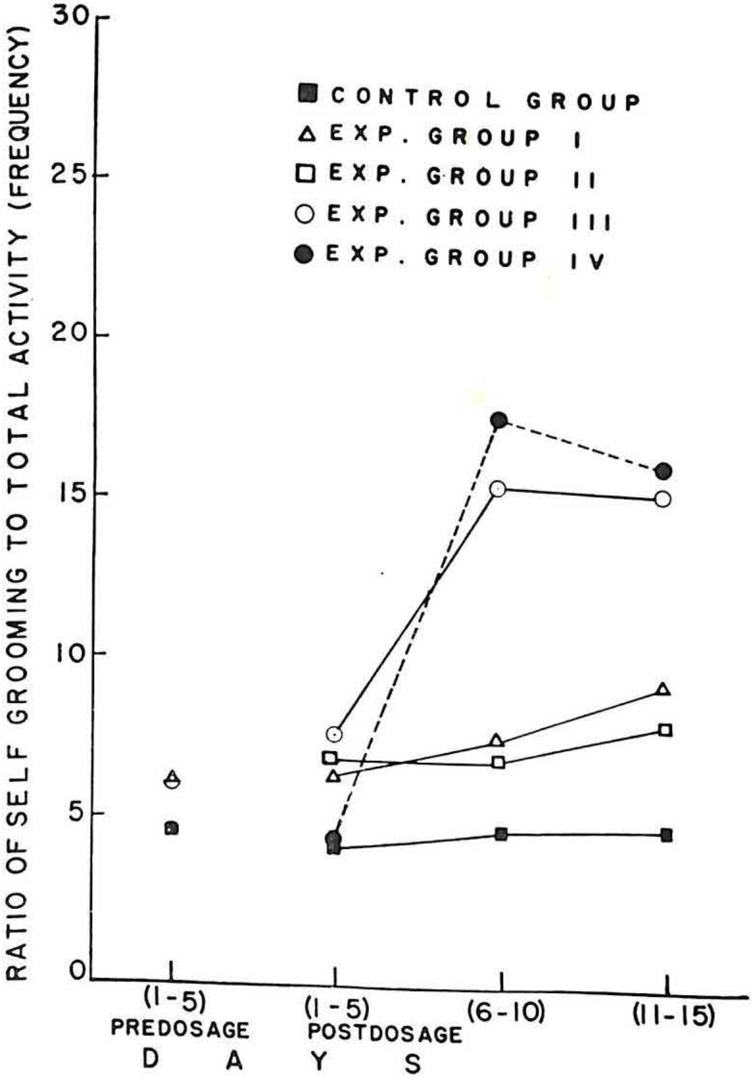

FIGURE 3-A
RELATIVE *Frequency* OF SELF-GROOMING ACTIVITIES BY NITROGEN MUSTARD TREATED AND NON-TREATED MONKEYS BEFORE AND AFTER THE PERIOD OF TREATMENT

Figure 6-A, and for longer periods, Figure 6-B, than *S*s in the control group ($p = .01$ except in the comparisons between the control group and Group II which are not significantly different). The *S*s in Group II rest significantly less frequently than *S*s in each of the other treatment groups. In general the *S*s in the treatment groups rest significantly more in the posttreatment periods than in the pretreatment period. The only exception to this finding is be-

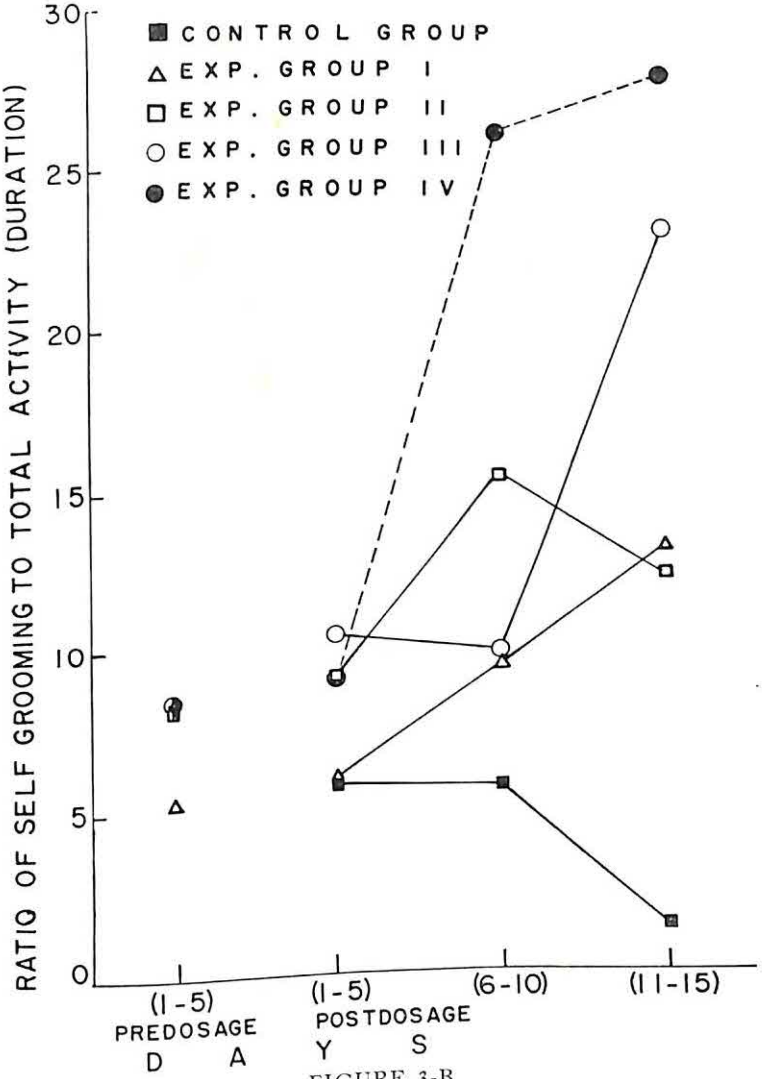

FIGURE 3-B

Relative *Duration* of Self-Grooming Activities by Nitrogen Mustard Treated and Non-Treated Monkeys Before and After the Period of Treatment

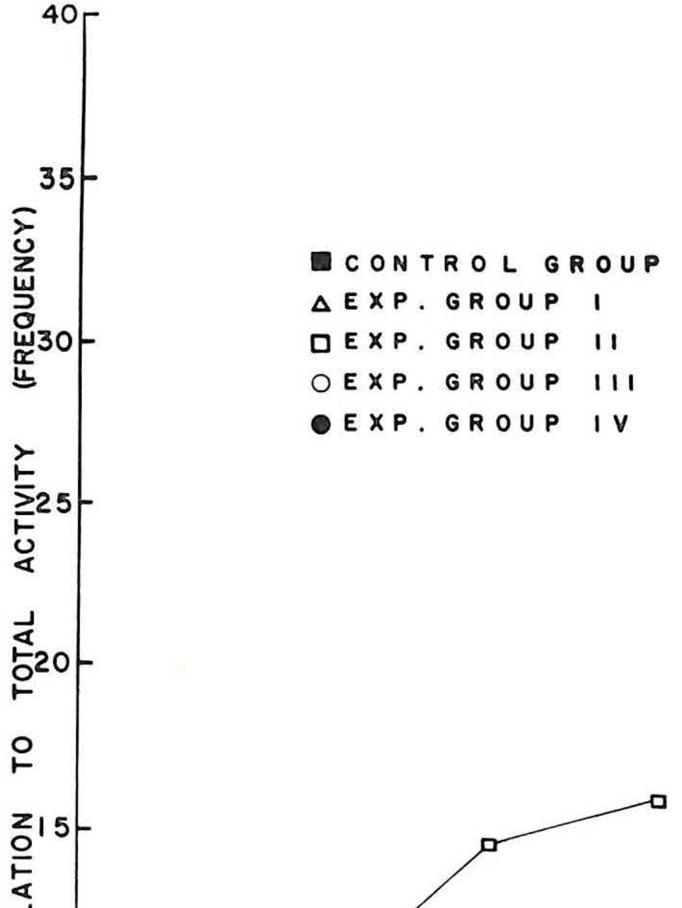

FIGURE 4-A
RELATIVE *Frequency* OF CAGE MANIPULATION ACTIVITIES BY NITROGEN MUSTARD TREATED AND NON-TREATED MONKEYS BEFORE AND AFTER THE PERIOD OF TREATMENT

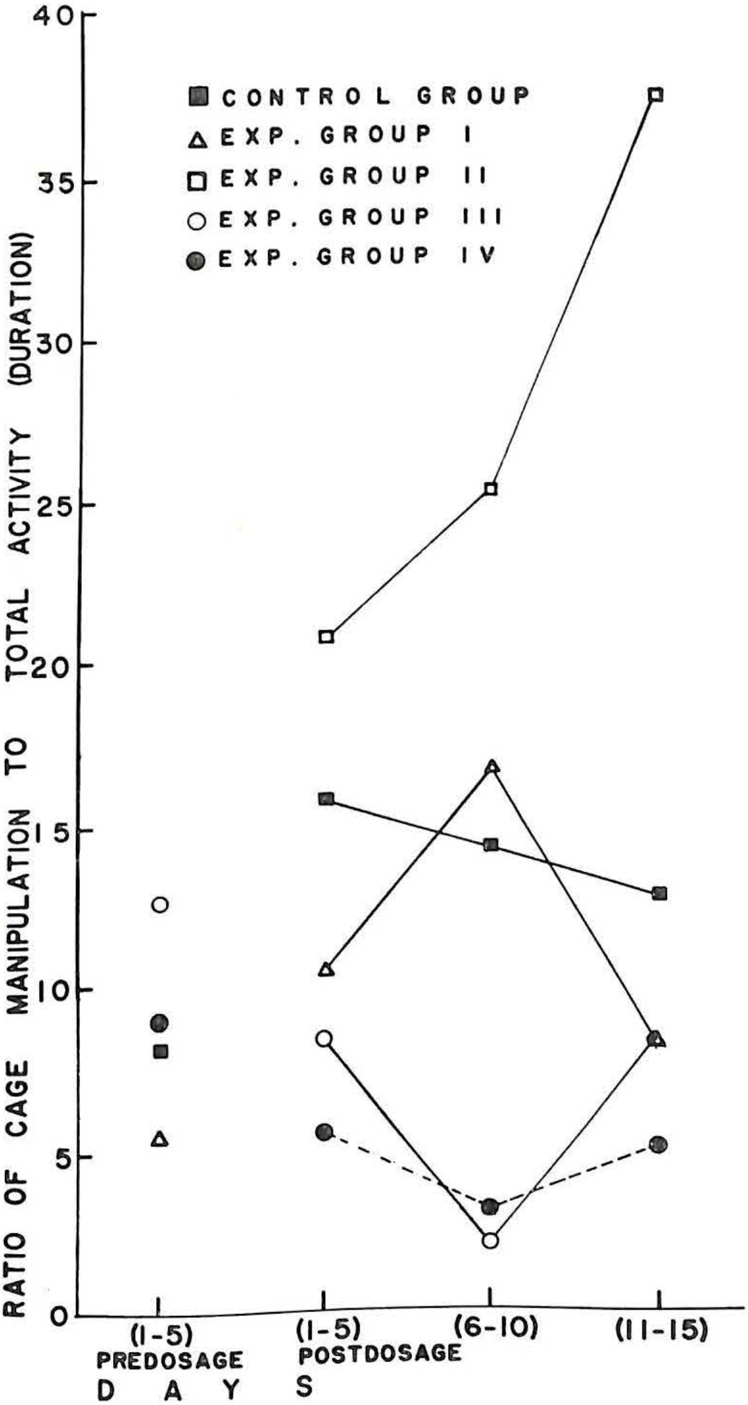

FIGURE 4-B

RELATIVE *Duration* OF CAGE MANIPULATION ACTIVITIES BY NITROGEN MUSTARD TREATED AND NON-TREATED MONKEYS BEFORE AND AFTER THE PERIOD OF TREATMENT

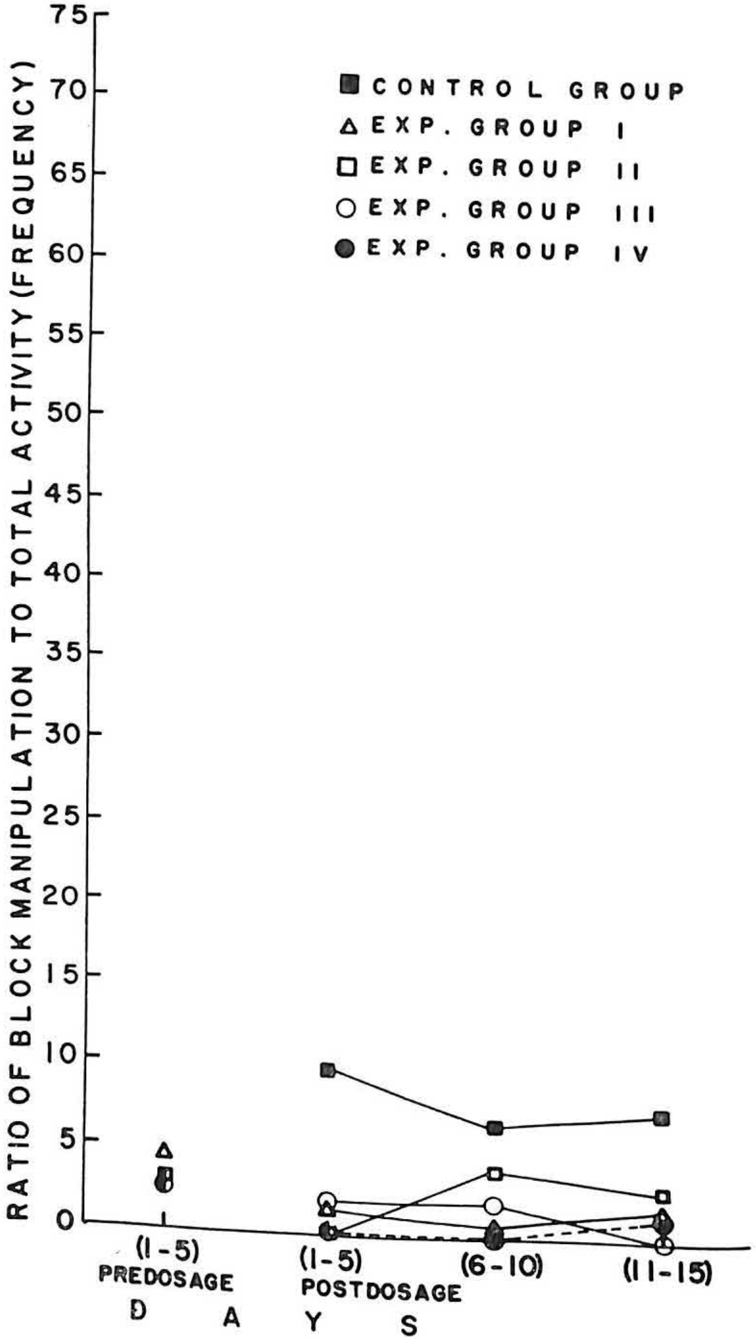

FIGURE 5-A
RELATIVE *Frequency* OF BLOCK MANIPULATION ACTIVITIES BY NITROGEN MUSTARD TREATED AND NON-TREATED MONKEYS BEFORE AND AFTER THE PERIOD OF TREATMENT

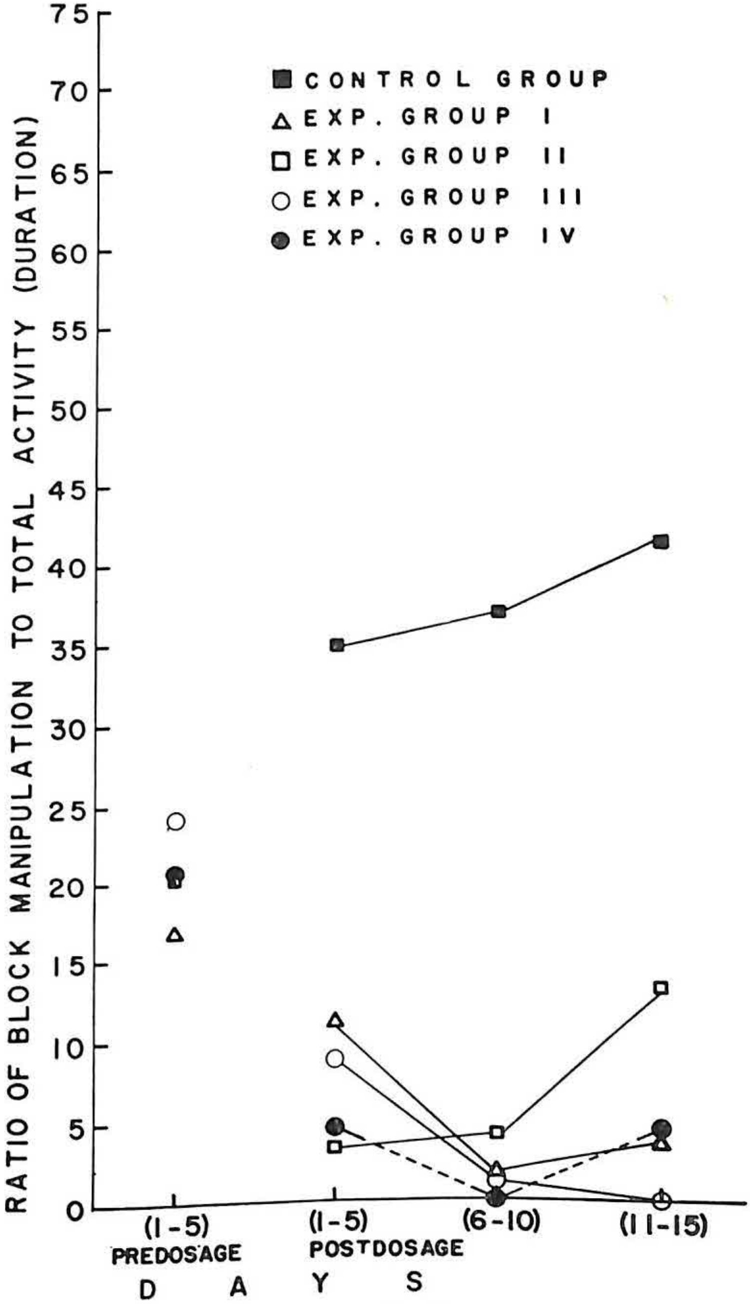

FIGURE 5-B
RELATIVE *Duration* OF BLOCK MANIPULATION ACTIVITIES BY NITROGEN MUSTARD TREATED AND NON-TREATED MONKEYS BEFORE AND AFTER THE PERIOD OF TREATMENT

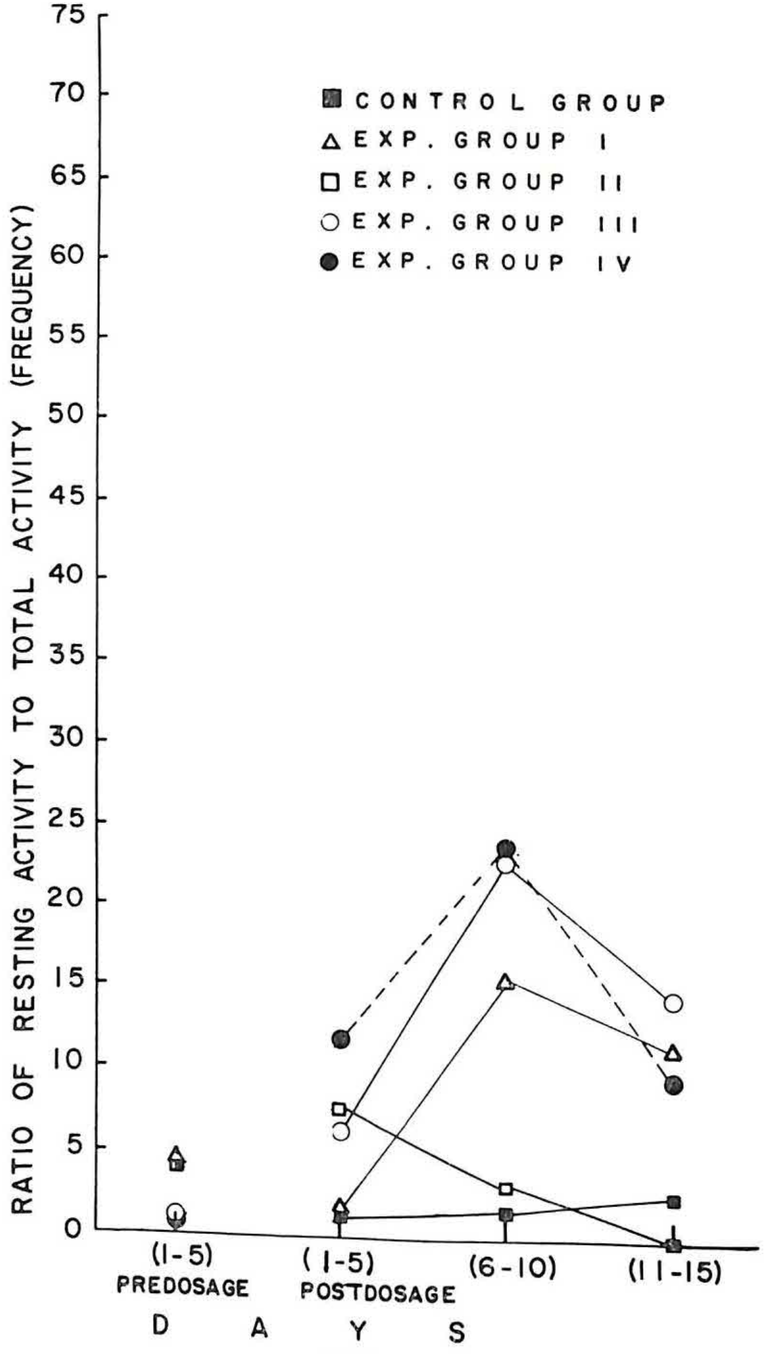

FIGURE 6-A
RELATIVE *Frequency* OF RESTING ACTIVITIES BY NITROGEN MUSTARD TREATED AND NON-TREATED MONKEYS BEFORE AND AFTER THE PERIOD OF TREATMENT

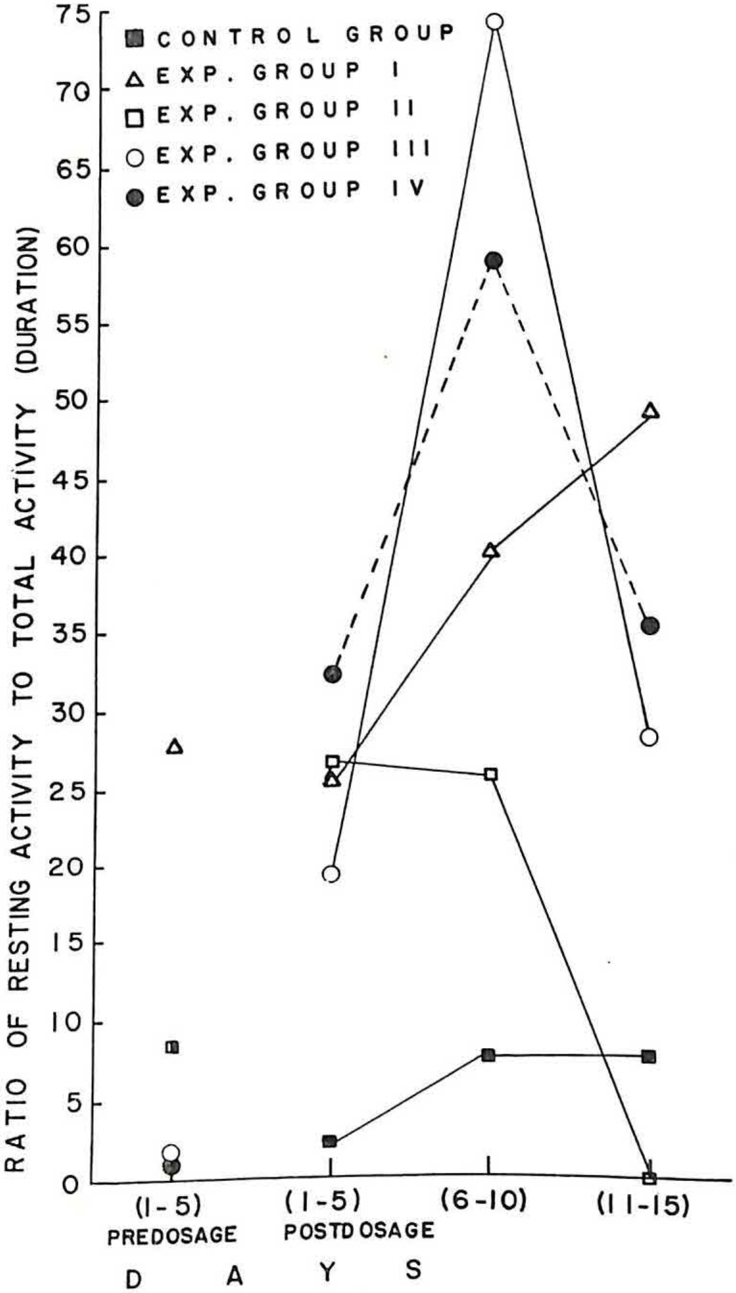

FIGURE 6-B
RELATIVE *Duration* OF RESTING ACTIVITIES BY NITROGEN MUSTARD TREATED AND NON-TREATED MONKEYS BEFORE AND AFTER THE PERIOD OF TREATMENT

tween the performance of the treatment groups measured as duration of resting in the pretreatment period and in the last posttreatment period.

g. Grooming and aggression. The data for both of these categories appeared too infrequently to make a full analysis possible. However, by testing the hypothesis that each of these activities would appear in a smaller proportion of the observations of debilitated *S*s than those of normal *S*s, significant differences were found for both categories. For grooming this difference is significant at the 5.0 per cent confidence level ($\chi^2 = 4.41$; df $= 1$), and for aggression significant at the 1.0 per cent confidence level ($\chi^2 = 40.51$; df $= 1$).

D. DISCUSSION

The results of this experiment indicate that the technique used in this study is a valid instrument by which behavioral changes due to the administration of nitrogen mustard may be assessed. Significant changes were found in each of the eight behavioral categories used.

Also, the nature of these changes precludes an interpretation in terms of a general decline in activity level only. There was no uniform, generalized deficit. Since the results were computed in terms of relative performance (each score was expressed as a ratio of total activity), the changes as demonstrated in this experiment represent experimental effects along psychological, rather than physiological, dimensions. The deficits which were found varied widely in degree, and in several cases (visual activity, cage manipulation, and self-grooming), significant posttreatment increments were shown. It may be concluded that specific features which characterize the behavioral syndrome resulting from nitrogen mustard poisoning have been isolated.

Three categories, resting, block manipulation, and location shift, showed in general the kind of reaction which would be expected of animals in a state of debilitation. Resting (Figures 6-A and 6-B) showed a consistent group increment which continued throughout the duration of the experiment. The performances for location shift (Figure 1) and block manipulation (Figures 5-A and 5-B) were severely depressed. However, as compared with posttreatment Days 6-10, some degree of recovery was apparent during the last days of the experiment for both.

The experimental effect on visual activity is unlike that shown in any of the other categories. Duration measures (Figure 2-B) show a degree of depression which is not accompanied by comparable frequency decrement (Figure 2-A). The increase in frequency of visual activity as a function of dose during the period Days 1-5 posttreatment and the complete reversal of effect shown in Days 6-10 reveal a dose-by-time interaction not duplicated in any other of the categories.

Of those categories which require some degree of energy output, only self-grooming (Figures 3-A and 3-B) and cage manipulation (Figures 4-A and 4-B) show any significant group increments. For self-grooming this increment is found in the performance of *S*s in each group during every posttreatment period. Although *S*s in three of the experimental groups show decrement for cage manipulation, *S*s in Group II (1.75 mg/kg) demonstrated an atypical increment. For the duration measure this increment was significant at the 5.0 per cent confidence level. Both Group I (1.00 mg/kg) and Group III (2.00 mg/kg) *S*s show decrements. This decrement for *S*s in Group III is significant at the 1.0 per cent confidence level. It is primarily due to the increment shown by animals in Group II that posttreatment Days 11-15 show large increments for both frequency and duration.

One study of X-ray radiation effects, McDowell, Davis, and Steele (5), is grossly similar to this experiment. Both studies employed observational methods to study changes in the free cage behavior of rhesus monkeys. Although the categories used are not defined in exactly the same way, it is felt that some comparison of the two syndromes may properly be made.

Although most of the results of nitrogen mustard poisoning are radiomimetic, there is one important difference between the two syndromes. For nitrogen mustard, the period of maximum effect generally appears during Days 5-9 posttreatment. For irradiation, the corresponding period occurs on Days 13-18 following exposure. This short latency of effect is also reflected in the mortality data. None of the nitrogen mustard treated animals died after Day-8 posttreatment, whereas one of the animals which had been subjected to X-ray radiation died on posttreatment Day-18. Since the temporal courses of the two syndromes do not coincide on a day-by-day basis, the intercomparisons will be made in terms of precrisis, crisis, and postcrisis.

Both syndromes show depressed scores in shifts in location. Except in the critical period this depression is somewhat more pronounced for the mustard treated *S*s than for the irradiated *S*s. The performance for the irradiated animals remained in the low normal range during both the precritical and postcritical periods, while the animals treated with nitrogen mustard showed a more pronounced drop during the precrisis, dropped still more during the crisis, and demonstrated a degree of recovery during the postcrisis; this recovery did not return to the pretreatment level during the progress of this experiment.

Cage manipulation behavior showed highly similar effects for each of the agents. The principal difference appeared in the precrisis where the animals treated with nitrogen mustard again showed the larger decrement. The animals subjected to X-ray radiation showed little effect until the critical period (Days 13-16) where a sharp drop comparable to that shown for shifts in location is found. After the crisis (highly similar for the two groups of animals) a rise in performance to a level higher than that which was demonstrated during pretreatment observations, is shown for both of the groups. For cage manipulation, as was true for shifts in location, the posttreatment effect is more pronounced during the precritical period for the animals treated with nitrogen mustard than for the irradiated animals.

Self-grooming data for the two syndromes show a striking similarity. For both the nitrogen mustard treated *S*s and the irradiated animals an increment begins to appear early in the precritical period, and this increment continues throughout the observations in each of the experiments. The maximum effect is found throughout the postcritical period for the nitrogen mustard treated *S*s instead of the period covered by posttreatment Days 6-10, and for the irradiated *S*s this maximum effect is found to appear during Days 21-24 instead of Days 13-16. Thus, for both the experimental variables, the effect (an increment) appears earlier for self-grooming than for the other categories, the effect remains for a longer time, and the peak of this effect comes later, relative to the administration of the experimental agent, than for any of the other categories.

Aggressive behavior showed posttreatment decrement for the animals of both experiments.

Grooming showed significant decrement as an effect of the administration of nitrogen mustard. Only two animals of the X-ray radiation study showed this decrement, whereas all the other irradiated *S*s demonstrated an increment in grooming activity. It may be important to note that the two animals to show decrement following X-ray radiation were the only male animals in the experiment; the females *all demonstrated increment.* Since all the *S*s used in the nitrogen mustard experiment were male, it is possible that a sex difference may be present.

It is unfortunate that there was not a greater correspondence in the design of the two experiments. Although the comparisons of the effect of nitrogen mustard and X-ray radiation are limited because of these differences, it is evident that the radiomimetic properties of nitrogen mustard extend to the behavioral dimensions.

The purpose of this paper was to present certain behavioral changes, not to explain them. The complexity of the underlying mechanisms would make such an attempt implausible. The value of any information which may contribute to a clarification of this problem is, of course, unquestioned; and a brief consideration of certain factors which may be pertinent may not be inappropriate.

That general physiological debilitation is a large factor underlying many of the behavioral changes seems to be certain. The white blood count is severely depressed, and the mocosa of the digestive tract is critically affected. These two effects alone would produce a marked lowering of physiological functioning. However, in addition to the physiological insult, it seems probable that motivational factors may also exert a considerable influence upon the altered behavioral patterns.

Jones, Kimeldorf, Rubadeau, Osborn, and Castanera (4) have shown that the volitional activity of rats did not follow concomitant physiological indices. Their ability to maintain activity (as measured by swimming-to-exhaustion) following X-ray radiation and their volitional activity (as measured by an activity wheel) failed to demonstrate a consistent relationship.

There appear to be a number of reasons why the adoption of nitrogen mustard as a standard experimental variable would be to the advantage of both physiological and psychological research. Its widespread use as a therapeutic agent makes it an important drug in its own right. Also, since the radiomimetic properties of nitrogen mustard apparently extend to the psychological, as well as physiological dimensions, it may well prove to be a valuable and convenient substitute, or supplemental technique to be used in conjunction with X-ray radiation research.

The experimental use of nitrogen mustard, as comparted with the use of X-ray radiation, offers the following advantages: (*a*) It can be used anywhere, since no bulky, expensive facilities or equipment are required for its administration. (*b*) The amount of dosage can be precisely controlled; the exact amount of actual, effective exposure is difficult to control when radiation sources are used. (*c*) Precise control of both the size and locus of the area selected for study is possible with nitrogen mustard; with irradiation some degree of exposure in depth, as well as in the vertico-horizontal dimensions, to tissues surrounding the selected area is unavoidable.

Perhaps the primary contribution made by the present study lies in the syndromes which it presents of the behavioral changes resulting from the controlled use of a known debilitating agent. It is to be hoped that these results, when evaluated along with comparable behavioral patterns of other known, and controlled, etiological factors, will be of value in the isolation and identification of some of the physiological mechanisms which underlie particular kinds of behavioral manifestations. If specific behaviorial patterns could be established as invariant with known physiological symptomatology, it would be of great benefit to clinical evaluation and diagnosis.

E. Summary and Conclusions

This study was conducted to investigate changes in free-cage behavior of rhesus monkeys as a function of differential dosages of nitrogen mustard. Twenty-four preadolescent male monkeys were used as subjects. Each animal was randomly assigned to one of four experimental groups, and each group corresponded to one of the four experimental conditions. Dosage was accomplished by intravenous injection and was computed in milligrams of nitrogen mustard per kilogram body weight. The dosages for the four experimental groups were as follows: Group I, 1.00 mg/kg; Group II, 1.75 mg/kg; Group III, 2.00 mg/kg; and Group IV, 3.00 mg/kg. Control data were obtained on the animals of Experimental Group II before they were subjected to the experimental variable.

Observational techniques were employed to collect the data. These data were recorded and tabulated in terms of eight preselected behavioral categories. These categories were shifts in location, visual activity, self-grooming, cage manipulation, resting, other animal grooming, and aggression.

Data were recorded by means of a keyboard of microswitches which were connected to time clocks, mechanical counters, and a kymograph. Frequency measures were taken for eight categories of behavior and duration measures taken for seven.

In order to insure that the results obtained would not be merely a reflection of general debilitation, the data in each category were presented as a percentage of the total activity of the corresponding period. Although significant changes were found for each of the eight categories, there was no uniform pattern. The evaluation of these changes included the functions of time as well as of dose.

Frequency and duration indices of performance changes tend to give similar pictures for most of the categories; in general, frequency tends to be a somewhat more sensitive measure. However, for cage manipulation, duration differences are more sensitive than frequency (1.0 per cent level and 5.0 per cent level respectively). Also, for visual activity, the patterns demonstrated by the two indices are quite dissimilar. For duration, a consistent decrement is shown following dosage, while frequency scores show both increments and decrements when compared with pretreatment scores.

Comparisons were made between the behavioral syndrome resulting from nitrogen mustard poisoning and the syndrome resulting from irradiation with X-rays as presented by a grossly similar study (5). The radiomimetic properties of nitrogen mustard were found to extend to the psychological, as well as the physiological dimensions; important differences were, however, established.

Coefficients of stability were computed for six of the categories and all were found to be significant beyond the 1.0 per cent confidence level, and significant behavioral changes were found in each of the eight categories used in the experiment. It may therefore be concluded that the technique employed is a reliable and a valid instrument by which to assess the behavioral changes which result from the administration of nitrogen mustard to rhesus monkeys in a free-cage situation.

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THE PYTHAGOREAN COMPLEX IN THE BEHAVIORAL SCIENCES*

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Almost all psychologists are aware of the fact that our tremendous faith in the possible efficacy of introducing scientific method into the domain of the behavioral and social studies, is largely due to the historical success of these methods in the natural sciences, themselves. Most psychologists are quite familiar with the fact that the revolt against philosophical dualism in science, and the ideological distemper, caused by the excesses of mentalism, were in fact part of the struggle against the domination of the world of learning by the church and by existing theological doctrine. Psychologists with an interest in the sociology of knowledge have run head-on into these considerations, in one form or another.

Less familiar, I believe, is the fact that the worship of mathematical methods has been equally conditioned by social circumstances, although this particular *idol of learning* has a longer history. Though the recognition of the importance of number does not begin with Pythagoras, the *systematic emphasis* on the notion that number lies at the heart of the universe, can be said to have begun with him. This emphasis we shall christen, with due poetic license, the *Pythagorean Complex* (PC). Like the faith in the methods of science, the PC also has its social determinants in that, having discovered the extent to which the laws of harmonics were expressible in mathematical terms, the Pythagoreans then extrapolated from these discoveries and took as an article of faith that similar mathematical regularities would be found to underlie all phenomena. With the rise of modern science, following Newton's amazingly successful theories, this faith has been strongly reenforced. As a result various brands of uncritical positivism have arisen in our time which take as an article of faith for intellectual inquiry, that the methods of the natural sciences assisted by the *functional thinking* of mathematics and various types of logical analysis, will suffice to reveal truth in all her glory. This faith has been increasingly borrowed by workers in the behavioral and social sciences over the last half century or more and, apart from the fact that it is nursed as carefully as though it were the heir to a throne, it must

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be admitted that it has inspired a large amount of research much of which has proven to be of real significance.

Mathematics has, indeed, been called the "language of structure." This phrase need not, of course, refer only to mathematical analysis. The language of structure includes the use of logical calculi of every sort and derivatively any type of formal analysis. A language of structure is either material or formal. If it is material it proposes to deal with experience by approaching it in terms of its public, objective, external properties—properties which are expressible in terms of space-time coordinates, thing-language, test-operations and test results, together with operational and measuring devices for dealing with all of these. If it is formal it is concerned with systems of related concepts whose referents may be either syntactical or material, but where each such concept, except for the protocols of the systems, can be sharply defined. To speak of a concept as sharply defined means that communicants have found it to possess so little surplus meaning, at least up to the present, that the tasks at which the concept is employed can be performed with little difficulty. It also means that the terms of the formal language have little, open texture and so little, fringe ambiguity that, on the whole, analysis proceeds smoothly. The preceding remarks, of course, represent some oversimplification but the amount of such oversimplification is appropriate, we believe, to the treatment of the discussion which follows and to the purposes of that discussion. The preceding considerations should then make clear those matters which are involved whenever we speak of the language of structure, provided that we understand that within this language the *ultimissimum bonum* is mathematics, itself. A strong conviction in the behavioral and social sciences, that the methods of the physical sciences together with the use of the language of structure, will be sufficient to advance our knowledge of man, is then, on our terms, a more recent aspect of the PC.

In our time this most recent emphasis has come to be dubbed "scientism" by those thinkers who believe that the Pythagorean faith is an insufficient underpinning for the development of theory and the prosecution of research in the behavioral and social science domain. The fundamental presuppositions of scientism are pithily described by Hayek (4) who has furnished its most systematic critique to date. According to Hayek "scientism" represents (pp. 15-16)

> "an attitude which is decidedly unscientific in the true sense of the word, since it involves a mechanical and uncritical application of habits of thought to fields different from those in which they have been formed. The scientistic as distinguished from the scientific view is not an un-

> prejudiced but a very prejudiced approach which, before it has considered its subject, claims to know what is the most appropriate way of investigating it."

The same author quotes a passage from Bridgman (1) which is meant to show how much natural scientists, themselves, take for granted an attitude which has somewhat stultified fresher and more appropriate viewpoints in the social and behavioral domains. Bridgman says (p. 46):

> It is difficult to conceive of anything more scientifically bigoted than to postulate that all possible experience conforms to the same type as that with which we are already familiar, and therefore to demand that explanation use only elements familiar in everyday experience. Such an attitude bespeaks an unimaginativeness, a mental obtuseness and obstinacy, which might be expected to have exhausted their pragmatic justification at a lower plane of mental activity.

Although Hayek is a philosophically oriented economist, it is worth the psychologist's patience to give him a careful reading for a variety of reasons but most of all for the brilliant manner in which he sheds light on the social and historical determinants of the scientistic credo. In this paper I wish to examine critically certain aspects of the scientistic credo, in relation to some of the problems of modern psychology. My greatest concern will be with what I take to be the insufficiency of the PC.

Perhaps the most naive and unexpurgated faith in the PC was exhibited by Clark Hull (5). It is contained in the epilogue to his *Principles of Behavior,* which represents a credo that has gone all out in this direction. The reader should, of course, examine Hull's apostrophes for himself, but one excerpt from that epilogue entitled "A Forward Glance," is worth quoting here (pp. 400-401):

> But we should not deceive ourselves. The task of systematically developing the behavior sciences will be both arduous and exacting, and many radical changes must occur. Behavior scientists must not only learn to read mathematics understandingly—they must learn to *think* in terms of equations and the higher mathematics. The so-called social sciences will no longer be a division of *belles lettres;* anthropomorphic intuition and a brilliant style, desirable as they are, will no longer suffice as in the days of William James and James Horton Cooley. Progress in this new era will consist in the laborious writing, one by one, of hundreds of equations; in the experimental determination, one by one, of hundreds of the empirical constants in the equations; in the devising of practically usable units in which to measure the quantities expressed by the equations; in the objective definition of hundreds of symbols appearing in the equations; in the rigorous deduction, one by one, of thousands of

> theorems and corollaries from the primary definitions and equations; in the meticulous performance of thousands of critical quantitative experiments and field investigations designed with imagination, sagacity, and daring to test simultaneously the validity of both the theorems and the primary principles and concepts from which the former have been derived; in the ruthless discard or revision of once promising primary principles or concepts which have failed wholly or in part to meet the test of empirical validation.

The excuse for the present paper is our conviction that a critical reexamination of *some* of the articles of faith of the PC is needed at the present time. Before plunging into criticism of the PC let us recognize unequivocally the great assets of this attitude. It has unquestionably extended man's knowledge in all spheres, *including* the behavioral and social sciences. It has created two sociotechnological revolutions of historical moment, the Industrial Revolution of the 19th century and what Norbert Wiener (17) has called the Second Industrial Revolution in our time. It has helped to dispel superstition and ignorance and break the shackles of intellectual errors which have long imprisoned man. It has provided thoughtways for the dispassionate examination of controversial issues, for which there are no substitutes. It has given us a picture of man and his world much more informative and accurate than that provided by any other approach one can name. On the practical side in the field of what has come to be called "operations research," it has given us a basis for social planning under any type of economy and any type of moral outlook, and a careful examination of a book such as that by Churchman, Ackoff, and Arnoff (3) will be convincing in this respect for the reader. Finally the role and applicability of mathematics, itself, to our various problems, both pure and applied, has in general proven itself beyond our most sanguine expectations. Nevertheless in several important respects the PC has proven *so far* to be a failure. This certainly has been true for many significant problems which center about conflicts in human values, both theoretical and applied. It has also been less successful in the effort to explain certain aspects of human behavior than one might wish. We shall discuss shortly some of the liabilities of the PC which have given rise to criticism of scientism in recent years.

In passing let us note that there is a complementary error to the PC. This is the methodologically uncritical and often behaviorally meaningless use of statistics, for which activity Sorokin (15) has coined the pungent labels "quantophrenia" and "metromania." For an extended exposition of the manner in which statistical method *may* degenerate into the worst abuses of scientism, the reader should acquaint himself with Sorokin's volume. Then there is the opposite, intellectual error to the PC, namely, the radical empiricist's idolization of data for which an appropriate, pejorative neologism might be "factophilia." The attitude of factophilia is most admired, I believe, by two types of psychologist. First, there is the psychologist who finds himself happy with any approach which gives comfort to a species of "tunnel blindness" by which he is enabled to avoid the really significant problems of the human condition. Second, there is the psychologist who seeks to achieve status by trying to convince both himself and the world that psychology is a genuine science. This type of psychologist tends to shore up this conviction by stressing a brass instrument approach. This approach is frequently accompanied by a distrust of theory, which has prompted Meier (11) to refer to workers of this type as lowbrows and specialists "whose greatest skill is often found at the tips of their fingers." This type of psychologist has so little personal flair for theory that he makes a fetich of the aimless accumulation of fact. However, it is the shortcomings of the PC which are our major concern in this paper. These shortcomings are not the fault of scientific and philosophical adventurers. They are intrinsic to the nature of scientific method and particularly to the language of mathematics. We wish therefore to turn our attention at this point to a discussion of the sense in which mathematics as a language may be irrelevant for some of the livelier concerns of psychology and the behavioral sciences in general.

Perhaps the most fundamental consideration which we must recognize is that mathematics is the *language of structure*. All aspects of experience which are not communicable through the language of structure or *not communicable at all*, obviously cannot be dealt with by it. In what sense do we speak of an experience as being structured? An experience is said to be structured if there are elements of it which can be made *public or intersubjective*, to employ a term used by the logical positivists. Such elements can be made intersubjective only if they can be placed into isomorphic relationship with *any* of the features of certain *abstract structures or languages*. Among these are types of number systems, number relationships, relationships of order and logical calculi, such as the calculus of classes or propositions. These, of course, are a small sample of abstract structures in use in scientific work today. Now are there in fact types and realms of experience which do not lend themselves to being structured in the sense in which we are employing this term? Quite obviously there are, and many of these are familiar to us all. Thus the interaction of light waves with the human organism yields *qualitative experiences* which we learn to name or signal as "red" or "yellow," etc. These experiences are clearly *entirely private* and are not *communicable via any language,* *least of all a language of structure.* These are protocol experiences which are *assumed* to have been shared by others when they employ the same name or signal. It is perfectly true that the presence of a wave length manifesting itself through the dispersion of light by a prism, may be made public through instrumentation, may be expressible as a parameter in an equation and may thereafter be analyzable mathematically. However, the experience which arises as a result of the interaction of the organism with the light stimulus, itself, is certainly not communicable via that same language and it is only a matter of *inference* that our neighbor's experience is similar to or identical with our own. Anyone who has had the surprising experience of cherishing an acquaintance who has for years responded gratefully to gifts of green ties, only to discover eventually that he had always separated his ties by color and that the part of the rack supposed to contain only green ties also had several red ties interspersed with the green, will appreciate the cogency of this point. Such protocol experiences, and we must reemphasize this, are *assumed* to be common. It is only their real or imagined physical correlates which we can make public with a language of structure, but never the experiences, themselves. Most psychologists are well aware that traditionally sensation-states, affects and a number of other psychological states, have been held to be strictly private. There are other realms of experience, however, which are likewise incommunicable by a language of structure.

Another such is the realm of *felt values.* One cannot describe a felt value *structurally* although one may employ language to bring *attention* to it. The best one can do is to *suggest* it or hope to evoke it in the auditor, through *expressive* language, but even when the auditor says "I see what you mean," it is only an *inference* that he does so and moreover an inference which will never be verifiable in a *public sense.* There is an aspect of everyday experience which should make this quite clear. The commonest type of *alienation* in human behavior is the so common situation in which an individual gives lip service to values of "tolerance," "democracy," etc., on which there is a reasonable consensus of opinion as to the *general dispositions* these values should arouse where they are honestly and clearly held. However, these *named* values frequently tend to degenerate into shibboleths. We then find that the same individual who gives lip service to these values is unable or unwilling to concretize them in specified settings. In other words the follow-through behavior in a setting which, it is commonly agreed, would reflect the *disposition* that is supposed to accompany the value, rarely appears. This socially tolerated and frequently socially approved schizophrenia, is a major result of the fact that values are incommunicable structurally. The distinction between a *represented* and a *felt* situation is perhaps well brought out in the following fashion. We would rely on a scientific or scholarly report to inform us intellectually of the *public* aspects of the tragedy created for farmers in the dust-bowl area during the depression of the 30's. Such a report is a structured situation. We would rely, however, on *The Grapes of Wrath,* a medium which employs *expressive, suggestive, evocative* and *empathic language,* to arouse in us a *proper disposition* towards the human victims of that structured situation. Furthermore we wish to arouse the proper disposition so that we can goad others into doing something about the situation. The sorrows of, and the tragic sense of life in, the human condition, have either to be imaginatively felt or affectively projected. They cannot be derived from mathematical models or other rational, abstract structures which give an accurate picture of the structural features of a situation, but which omit the *impact* of that situation on the actors within it and delete from record the role which feeling and value play within it.

Although a value context is not involved in the following examples, the egocentric predicament, of course, is even worse if we seek to *express* some experiences for which our auditors have no apperceptive mass, whatsoever. The quality of sex experience is probably not even suggestible to the victim of the Fröhlich syndrome. A particular type of taste sensation, foreign to a subject's experience, is not suggestible by any type of linguistic approach. The schizophrenic-like states experienced by a medical experimentalist who has just emerged from a siege with *lysergic acid*, is only barely suggestible to normal subjects, via the arts of communication. And if there are emergent experiences, that is, experiences which heretofore have been vouchsafed to no one but which at this time are either shared by a few individuals or are possibly unique to one person only, there is simply no way to suggest this to others at all. To *read* about the effects of hashish, marijuana, or mescalin, even when the reading is aided by the most vivid imagination possible, is *extremely remote* from directly experiencing the effects of these agents, themselves. What is true of these egocentric predicaments is *a fortiori* even truer of states of *felt* values, in spite of the fact that the words we use to *express* these, are familiar to us all. To stress all these considerations is neither to retreat into a mystique nor to exhibit a "failure of nerve." It is simply a reminder of the obvious which tends to get overlooked when structural habits of thinking and perception become stereotyped. These habits are clearly professionally conditioned, although little effort is made to determine boundaries in experience beyond which they are inadmissible. There are other types of experiences and states for which a language of structure is irrelevant, but we will pass over these here.

The recognition that the realms of feeling and value, not to mention other states of Being to which we will allude shortly, lie wholly outside of the type of approach the PC can provide, has been increasing in recent years. Major recognition of those concepts and experiences which are neglected by psychologists guilty of the methodological error of the PC, has been furnished by those members of the profession who call themselves *orthopsychologists* and whose outlook parallels that of Maslow and Sutich (9). In addition a supplementary organization concerned with these and kindred matters is the Research Society for Creative Altruism,[1] founded by Sorokin. The point of view of the American orthopsychologists can reasonably be said to represent a wish to deal with the non-structural aspects of experience. At a time when plans were being laid for a new journal, Maslow and Sutich had this to say.

> The *Journal of Ortho-Psychology* is being founded by a group of psychologists who are interested in those human capacities and potentialities that have no systematic place either in positivistic or behavioristic theory or in classical psychoanalytic theory, e.g., creativeness, love, self-actualization, "higher values," ego-transcendence, objectivity, autonomy, responsibility, psychological health, etc. This approach to Psychology can also be characterized by the writings of Goldstein, Fromm, Horney, Rogers, Maslow, Allport, Angyal, Buhler, Moustakas, etc., as well as by certain aspects of the writings of Jung, Adler, and psychoanalytic ego-psychologists. . . .
>
> We are especially interested in the explicit application of psychological knowledge to the rapid development of a usable, naturalistic system of human values, a task now avoided by most academic and clinical psychologists. We remind these psychologists that this task is a time-honored one as the history of psychology will clearly show. Abdication from this problem has been a recent development, at best only temporarily justified by the need to establish psychology as a science. That psychology has been fully recognized by responsible people everywhere as a science for at least a decade is no longer questionable. But that it should continue "to lean over backwards" in order to maintain that status and in so leaning reject vital areas of psychological research and inquiry is quite another thing. Therefore, we feel very strongly that psychologists can and should resume as one of their basic functions, their ethical and historical task of studying scientifically the optimal or

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[1] Among the speakers at the first conference of the Society were: Gordon W. Allport, Ludwig von Bertalanffy, Theodosius Dobzhansky, Robert S. Hartman, George Kepes, Lawrence S. Kubie, Dorothy Lee, Henry Margenau, A. H. Maslow, Pitirim A. Sorokin, Paul J. Tillich, and Walter Weisskopf. Jacob Bronowski and Erich Fromm have also shown an interest in the work of this organization. Its first conference took as its aim a report to the public on work in the broad field of human values (moral, spiritual, aesthetic, economic) and their application to the affairs and institutions of mankind.

> highest possible functioning and improvement of the human being. . . . (pp. 1-3).

The orthopsychologists, being clearly aware of the limitations of the PC, cite with favor the following criticism of scientism, by Bugental (2):

> Increasingly the three major publications representing clinical psychology (Consulting, Abnormal and Social, and Clinical) seem to be given over to a kind of particularistic, numerological paper which reflects, it seems to me, a distinctly limited aspect of the broad area of the field. Indeed, at times some of these papers make one suspect that many of the writers are sort of a more sophisticated or learned breed of stamp collectors.
>
> Without attempting to debate further the relative worth of these contributions, it may be noted that as an organization devoted to "the advancement of psychology as a science and a profession" we tend, publication-wise, to emphasize what seems a limited aspect of the science.
>
> These thoughts seem to point strongly to the need for some channels wherein psychologists working in the more exploratory phases of our science and the professional aspects may communicate their discoveries and hypotheses (p. 2).

In several parts of the United States today independent psychologists have recognized not only the limitations upon the objectives of research set by the PC but also the need to combine both the idiographic and nomothetic approaches if we seek a true picture of man and adequate methods for the alleviation of the human condition. One of the more interesting pleas for an integration of outlooks is a paper by van Kaam (16), which the reader will find most illuminating. Reports on experimental procedures and findings in the work of the orthopsychologists are appearing at an increasing tempo. Three studies by Maslow (8) of direct interest to those psychologists concerned with the factors which allow the individual personality to reach its fullest stature, can furnish some idea of how the inadequacies of the PC can be outflanked, so to speak. But Maslow has also shown how the orthopsychological approach can deal appropriately with the large-scale, significant problems which cut across personality theory and power relations in political science. This is a hybrid area which the well-intentioned but minuscule approaches of a limited scientism, handles with the utmost difficulty.

Let us return, however, to a further discussion of the limitations of the PC. The initial reaction of a psychologist steeped in the nomothetic and scientistic attitude, to problems of value, is generally an effort to see *felt* value and the *expressive* declarations which are consonant with them, as verbal indications of organic needs or of learned interests which may have become functionally autonomous. These needs and acquired interests may be satisfied by long-circuiting operations. A viewpoint of this sort represents an extremely limited notion of what a value is. The complexity of evaluative and expressive activity and of the verbal declarations associated with these, as Kecskemeti (6) has tried to show, is far greater than this oversimplified picture would lead one to believe. Values, for instance, may be *completely* unrelated to *presently operating* organic or acquired needs. What Morris (12) calls *conceptual values* may represent an ideal intended to give direction to *evolutionary levels of aspiration*. By this we mean that they may express a wish for a state of affairs somewhat remote in time but which is regarded as more idyllic for human development than various alternatives equally superior to man's current estate but which are seen as inferior to that choice nearest to the evaluator's heart's desire. Utopian thinking frequently takes this form. Such projected ideals may be advanced by individuals who are not only content with their mortal lot but who may, along with Dr. Pangloss, see this as the best of all presently possible worlds. In such a case it would hardly do to see the projected ideal as one which meets the needs and frustrations of other men and then to argue that the desire upon the part of the Utopian thinker to satisfy the needs of others and to eliminate their frustrations, is, itself, an acquired need. Nor can it be argued, except by a monstrous stretching of the usual meaning of the phrases, "organic needs" and "acquired needs," that the projected ideal represents the expression of the needs of its formulator, but displaced in time, for the time displacements are usually so great in such imaginative, Utopian projections, that their formulators would long since have had to be dead before the ideal ever became reality. It is quite possible to fail to see the *altruistic features* of such projected, large-scale values, by a tortured accounting for their genesis in terms of physiology and learning theory, but we believe all such efforts are futile and miss the point. Often the projected state of affairs is so completely irrelevant to the subject's current needs and represents so dispassionate a point of view, that were the subject to be plunged by some magic into the midst of a world that possessed the projected attributes, he would not only not know how to adjust to such a world but its ideal aspects might be traumatically frustating to his present habits. This tendency to project evolutionary levels of aspiration is probably what Roback (14) had in mind when he defined normal behavior as behavior which will be said to be absolutely normal if we can assume that it will be ultimately proved right by continuing human experience. The difficulty with Roback's statement, of course, is the ambiguity surrounding the term "right," although this could be defined in any one of several ways which would minimize, if not reduce, the ambiguity. The long and short, however, of the orthopsychological position is that from its viewpoint the PC represents an oversimplified procedure for dealing with the complex issues and projected states of affairs, which problems of value entail. In this area Gordon Allport's concept of *intention* is indispensable.

We can focus the arguments against the limitation of the PC when applied to the context of value, by framing our objections somewhat more technically and with greater precision. Suppose for the moment that we accept the appropriateness of a personalistic and phenomenological point of view with respect to an individual's value-system. On the strength of such a position we would then assert that values are internal valences which direct behavior and prescribe both proximate and distant goals. Having done this we would note that any value possesses *at least* four attributes, two of which involve the language of structure and two of which do not. The existence of a value will make itself felt in terms of *value consciousness, value dicta, value meaning,* and *value assonance.* Let us briefly differentiate among these four considerations.

First, before a disposition, in the technical sense of this word, is called a value, a subject should be completely aware of it. A tendency to act in a given way in the presence of present or possible stimuli is a *valence* but it is not a *value* unless fully present to consciousness, and if fully present to consciousness in a manner which relates the tendency to its possible objects through the concurrent influence of emotional charge, then and only then can we speak of it as *felt.* When this state obtains we can speak of a subject as possessing *value-consciousness.* Second, if a value is genuinely felt, it may be *expressible* in words, although such a verbal report or any series of such verbal reports, can only be the linguistic *indicators* of a felt value but they cannot be its *definiens* because of the *affective* aspects of felt values. However, even the capacity to be an *indicator* is not necessarily assured by a statement, for an observer may judge that there is nothing in the behavior of a speaker which appears to correspond to the *meaning* of a given verbal report of a value. We should therefore have to speak of the *unanalyzed verbal report* as a *value-dictum.* This then naturally leads to the third attribute surrounding the notion of value, namely, what is the stipulated, public agreement concerning what the value-dictum entails for specifiable stimulus contexts. The most important of such contexts are those which involve our relations with other people and our attitudes towards what the sociologist calls our *self-images.* Without intersubjective agreement on entailed behavior there would be no advantage in hoping to predict a subject's behavior from a knowledge of his value-dicta, assuming these value-dicta were faithful indicator's of a subject's value-consciousness. We shall then mean by *value-meaning,* the public agreement concerning the behavior to be expected from the subject in a given stimulus context or by virtue of a publicly constructed inference concerning the self-image of a subject. The last attribute, however, in relation to a value situation, is the most important one. This involves the question of whether a subject's actual behavior is a *demonstration* in an unequivocal sense of the value-meaning of a value-dictum. If it is, we can speak of the behavior as being *value-assonant,* if not, *value-dissonant,* with the value-meaning. To what extent then is the language of structure applicable to each of these four attributes?

Inasmuch as value-consciousness, as a *dispositional value* in the sense in which I have already employed this term, is literally unobservable and inasmuch as the presence of its affective concomitants are inferrable but not verifiable, clearly the language of structure is not only irrelevant but it is also inapplicable for any universe of discourse concerned with value-consciousness. Although a value-dictum is publicly receivable and its value-meaning publicly analyzable so that all may agree upon those referents in behavior which it entails and upon the contexts of "requiredness," in Kohler's (7) sense of this term, to which such behavior would be truly relevant, the determination of the correspondence between a value-dictum and the value-consciousness which has presumably generated it, is another matter. This *value-isomorphism,* is not confirmable by observation nor is the language of structure designed to make confirmations of this nature. We are boxed in by the fact that only the individual who utters a value-dictum and who understands and intends its public, value-meaning, is in a position to confirm this isomorphism. Here again, because of the subjective nature of value-consciousness, it is clear that the language of structure was never intended for use in making confirmations of this nature. Furthermore there is an additional difficulty. If we accept any "mentalistic" position which recognizes, as with psychoanalysis, that *conflict* is a relation between two or more states of consciousness or between states of consciousness and states of the "unconscious," then communication is even more strongly blocked. In such an eventuality we have no right to assume with certainty that the *subject, himself,* is in a position to make what we might properly call "the solipsistic confirmation." In this case *no one* would be in a position to confirm the isomorphism while even its imputation by inference cannot be realized because the number and kind of conflicts in the subject, are unknown to the observer, while if the observer guesses at these he is not in a position to verify or confirm these guesses. The language of structure is therefore again irrelevant.

The value-meaning of a value-dictum is, of course, conventional and its establishment is a public operation to which the language of structure is clearly and definitely applicable. The task is essentially one of logical analysis. Determining the *value-assonance* or *value-dissonance* between the value-meaning of a value-dictum and the subsequent behavior of the subject who asserts the *value-dictum*, involves strictly an operation of verifiability which is completed solely through a series of operations. The observed behavior is or is not subsumable under the public, value-meaning. For this task the language of structure is definitely relevant. However, we should not lose sight of an unhappy difficulty which may occur in this context. The isomorphism between value-meaning and behavior may be completely independent of the isomorphism between value-consciousness and value-dictum. This can occur for several reasons. I shall mention two of these here. A subject may entertain a value-meaning which is not the public one. At the same time he may be characterized by so great a state of value disorganization that he may exhibit behavior which is assonant to the public value-meaning while possessing a value-consciousness which is isomorphic with the private meaning of his value-dictum. If he is not closely questioned concerning his behavior the existence of these independencies may not be realized. A second situation of the same sort can occur in the trivial case in which the subject's value-dictum is a consciously flagrant lie but, for one reason or another, usually political, he deliberately produces behavior value-assonant to its public meaning.

From an idiographic and phenomenological standpoint it would not be exaggerating matters to state that most of our distinctively human, that is, *symbolic* behavior, is dependent more upon states of value-consciousness than upon observable, molar behavior of the input-output variety. We must therefore reemphasize the inadequacies of the language of structure for dealing with these. What is it then that psychologists most frequently do in pursuing either discussions of value or in doing experimental work in the field of values. If we analyze our behavior closely we will find that it will generally fall under one of the following categories. (*a*) We examine and classify the reported value-dicta of our experimental subjects and try to relate these reports to other considerations, without trying to analyze the meanings of the value concepts involved in such reports. (*b*) In philosophical or methodological discussion we may employ various types of syntactical operations in order to arrive at public, value-meanings for given value-dicta. (*c*) We furnish descriptions of a subject's behavior and from these we try to construct inferentially those value-dicta which possess meanings that are value-assonant to such behavior. (*d*) We solicit *reports* from a subject, as in psychoanalysis, in order to infer (not confirm) what his value-consciousness may be. However, observers have no way of *directly* knowing a subject's value-consciousness or confirming it and for these reasons structural language will fail us here. Furthermore, we have no known means for inducing in others states of value-consciousness which we, ourselves, should like to bring about. In this connection I do not regard brain-washing techniques as falling into this category and for the following reasons. Meerloo (10) reports how superficial such presumed states are and how easily these fall away when the brain-washee escapes from or is released by his captors, and returns to freedom. Second, since brain-washing represents simply opportunistic conditioning, it lacks the property of being "fully present to consciousness in a manner which relates the tendency (to act) to its possible objects."

A logical positivist like Pap (13) who adopts a non-extreme form of the physicalistic thesis, will oppose the type of argument we have given above, which asserts that the language of structure is not designed for dealing with value-consciousness. He will oppose it on the ground that "it is possible to discover for any psychological property a correlated physical property such that predications of the psychological property are testable in terms of observations of physical events." Pap's argument runs as follows.

> If the presence of mental states or dispositions is to be tested, the test-operation is a stimulation of the organism, and the test-result is a response of the organism. A physicalist would not (or, at least, should not) maintain that "honest," for example, is *synonymous* with some physical predicate. To save his thesis, he only needs to show that predications of this psychological term are susceptible to some such behavioral test as is expressed by the following reduction-sentence: "If *x* is asked a question and *x* answers what he believes to be the case, then *x* is honest." Of course, this reduction-sentence calls for further reduction, since "belief" is itself a psychological term. How, then, could a sentence like "*x* believes it will rain" be reduced to behavioral terms? In somewhat the following way "if *x* is induced to leave the house, and *x* takes an umbrella with him, then *x* believes it will rain." Here again, no definition of the concept of belief is provided, but it is indicated how sentences containing "to believe" as their verb could be tested. By adding more and more reduction sentences, psychological terms are made more and more precise, and surely they need this even more than physical predicates need it . . . (pp. 292-3).

Clearly, of course, the *felt* values of states of value-consciousness, are psychological properties of the organism. However, my own argument depended substantially upon what I called the "concurrent influence of emotional charge," a consideration which appears to be neglected in Pap's argument. Furthermore, even if Pap were to admit that the presence of an appropriate, affective state is a necessary consideration in these matters, there is still no way of providing a firm reduction-sentence to indicate the presence of a feeling or emotion. This is because there are many ways to express an emotion in behavior, while, at the same time, different individuals express different emotions in the same way. To make matters worse it is hard to distinguish a simulated *expression* from a genuine one. However, let us meet Pap's argument on its own grounds, by treating a psychological state as though it were strictly cognitive. Let us then try to see if his argument goes far enough, at least with respect to the context of value.

The values (mental states) in Pap's argument can have their behavioral correlates expressed in reduction-sentences. These reduction-sentences are statements which express the value-meaning of the subject's value-dicta, whether expressed by the subject himself or expressed adequately for the subject by someone who is analyzing the value context. These statements of value-meaning, however, lead to certain difficulties. First, let us remember that a subject's value-consciousness may be one thing, while his value-dicta and their value-meanings may refer to something else again. For a variety of reasons this can occur without interfering with the possibility of his behavior being value-assonant to the meanings in question. In layman's language we are accustomed to calling this hypocrisy or pretense or accustomed to designating such a situation by some other label, but no matter by what name it is called, the label indicates that we recognize that the behavior in question may mislead us into imputing erroneous psychological correlates for the subject. Second, many values are *complex* in nature as, for instance, our private projections of the world as we should like to see it in order to realize some Utopian ideal. The realization of such values which are high-order abstractions, involves what the psychologist calls long-circuiting behavior. The effort to produce reduction-sentences for high-order, value abstractions of a complex type will generally involve either too great a plurality of possibilities for prospective behavior or occasionally too much indeterminancy, to make the task as easy as it was in the case of the very simple examples used by Pap. One is tempted to say that, *in principle,* this task could be done. Its execution, however, would have to be incomplete for the following reasons. High-order, value abstractions are elliptical in nature. Their equivocality is eliminated only by concretizing them, that is, framing reduction-sentences for them, when a specific context, circumstances, or social framework is supplied, in which these *value-complexes* are to be made univocal. Such ideals or value complexes take on variable, concrete forms with changing circumstances over time, so that one could never produce a set of reduction-sentences at any given time which were the value-meanings required. At best the analyst could produce these reduction-sentences in *ad hoc* batches, from time to time, as he was given a description of the social and physical contexts in which the behavior correlates of the psychological properties were expected to operate.

The indeterminancy of a structural analysis comes from the following fact. Inasmuch as there is likely to be a plurality of behavioral correlates each of which can be incorporated into a proper reduction-sentence which is part of the sentence-aggregate which spells out the value-complex, an analyst would be reduced to asserting the following. Given a knowledge of a subject's value-consciousness (and we have said nothing about this consideration, so vital and so difficult to ascertain in talking about the prediction of individual behavior), than the following statement is true. "*S* will do either *A* or *B* or *C* . . . or *Z*." If, however, we are forced into compound propositions of this sort, the type of successful predictions we obtain will not only be trivial but what is even worse they will not enable us to achieve social control, if by "control" we intend the meaning which is usually attached to the methodological demand for "predictability and control." Finally let us note that the use of a structural analysis in dealing with a value context, leads to a derived difficulty. When a subject's behavior is not assonant to the reduction-sentence (value-meaning) which purports to be the *prospective,* behavioral correlate of his psychological (value) state, how can we tell which of the following is true: (*a*) the dissonance is an indication of the value disorganization of the subject; (*b*) the dissonance is only apparent and is probably a result of an *erroneous* reduction-sentence for the psychological property (felt value); or (*c*) the dissonance occurs because of an *erroneous* construction of the subject's value-consciousness.

Considerations such as those involving the preceding distinctions which we have made for the context of value, should prompt us to try to distinguish when and where in that context the language of structure may be given legitimate entry.

The devotees of the PC also fail to recognize another basic consideration when dealing with the problem of value. This is the role played by the "inner conversation," that is, by the dialogue with oneself, as it were, when dealing seriously with complex and conflicting problems of value. This inner conversation which clearly only human beings are capable of, is essentially an introspective scanning device which, if carefully and rigorously pursued, leads to a more complete description of all the aspects of a value problem. When we say "carefully and rigorously pursued" we mean that such an inner conversation conducted honestly and with a willingness to face painful alternatives, can be converted into a sort of clearing house for values. A clearing house operation of this sort will present clearly all the cognitive difficulties with which a value problem bristles, will *express* all the affective, altruistic, and humanistic considerations involved and, if the situation of another person is our concern, will arouse all those empathic accents and sympathetic behavior without which a further pursuit of a solution or an appropriate attitude, would be both meaningless and impossible. This type of introspective scanning has given rise to the best products of literature, philosophy, and religion and the processes involved in such inner conversations need not be mystagogic or obfuscating at all. Such processes unfortunately cannot be described at present via a language of structure, but this is no criterion of their importance, for felt values and other non-structural attributes of experience are meant to be *suggested* or *evoked,* rather than to be linguistically structuralized. The question of whether a suggestion has been accurately received or a feeling-tone accurately evoked, can never be answered. That an appropriate mood has been evoked or a relevant feeling-tone conveyed, it must be reemphasized, must always be inferential, and therefore open to question. Yet the most important things in human life and the most important problems of the human condition, involve for their proper treatment, the inner conversation. This is, of course, consistent with the fact that in many cases some of our decisions may have been effectively mediated by the language of structure, insofar as such langauge is employed in the service of defining means-end relationships which have themselves been dictated by unstructurable *felt* values.

In dealing with those areas of experience which are not appropriately handled by the PC, we require a language whose most basic attributes would be a well developed *expressiveness,* a sensitive capacity for *mood evocation,* and the ability to evoke much needed empathy and sympathy in interpersonal relationships. These qualities are particularly needed in dealing with common goals and in the process of trying to transfer (not represent) a sense of value. To date the literary, graphic, and tonal arts have achieved a very clumsy approximation to these attributes. Humanity has never set itself the goal of constructing such a language, possibly aided and abetted by accompanying expressions of vocal tonality,[2] but such a language is more desperately needed than Esperanto, Interglossa, or any other proposed international vehicle of communication. The price we would have to pay for adopting such a language would be its almost complete impotence to convey the structural aspects of experience, should we ever make the mistake of trying to employ it for that purpose. If we are intelligent, however, we will adopt two languages, one for structurable and one for unstructurable experiences. If we can make clear the domain of proper relevance for each, the ambiguity of shared experience can be minimized. The demarcation of domains will not always be an easy task. Such an orthopsychological language is, however, a necessity when we need a medium for producing value-catharsis and when we seek to instigate altruistic behavior in others. The language of science and mathematics *must always* be used for the descriptions and regularities of public experience. We must not confuse the two. If I may be allowed a salty commentary, we cannot make love by writing equations nor can we solve equations by kissing them. Of one thing we can be sure. The PC has never, cannot, and will never be the appropriate medium for dealing with areas of experience which are intrinsically non-structurable.

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[2] The Chinese language, I am given to understand, does make use of certain types of vocal tonality to achieve certain types of interpersonal understanding and personal expression. Inasmuch as I am unfamiliar with that language I am unprepared to say whether felt values are more easily *transferred* in Chinese than in any of the Indo-European tongues.

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THE PLACE OF PROJECTIVE TECHNIQUES IN THE UNIVERSITY CURRICULUM: THEY DO BELONG THERE*

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The title of this paper is open-ended since it presents broad vistas for exploration. One must wonder, though, whether the real issue is training in the university versus training in the practicum situation for the clinical psychologist. Why does this question arise with reference to the projective techniques only? Why not with regard to projective theory, or personality theory, or intelligence testing, or even psychopathological labeling? May there not be justification for insisting that since clinical psychologists deal with troubled persons then clinical psychology trainees should learn all applied aspects of their profession in the best place for it—in the clinical facility, the haven for troubled people. It is not compelling to view the issue as one of either-or. Learning in the university classroom and in the practicum setting need not be discrete events but continuing and interwoven experiences. Why not consider university courses in projective techniques and the application of these procedures in the practicum under didactic supervision as two sides of a single coin?

A review of the recommendations by various sub- and *ad hoc* committees of the American Psychological Association which have studied the problems of training in clinical psychology at the graduate and internship levels furnishes a wealth of expert and near-expert opinions. In 1945 Shakow (5) noted four approaches in clinical psychology which might be helpful for thinking through the values in conjoint academic and practicum training. Two of these belong primarily in the university curriculum—the dynamic and experimental approaches. In the dynamic, the interest is focussed on understanding the development of human motivation and personality organization. Courses in the psychodynamics of behavior, motivation, psychopathology, and perception form the core for introducing the student to these essential concepts. The experimental approach is imparted directly in graduate courses and seminars centering around specific and general topics in personality theory, develop-

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[1] This is a modified version of the paper given at the joint Division 12—Society Projective Techniques symposium at the APA convention, August, 1958, Washington, D. C.

mental psychology, and assessment procedures in addition to those mentioned above. In each of the courses and seminars the attitudes engendered and encouraged are a function, in part, of the instructor's own approach to the subject matter. The university climate usually fosters the experimental-research view. Projective skills taught in the university department within the framework of the dynamic-experimental approach could accomplish three ends: First, to discuss with the student the theoretical assumptions underlying the procedure; second, to train the student in the techniques of administration, scoring, and interpretation of the evaluative tests; and third, to expose the student to the experimental literature for his continuing consideration of the areas of agreement, controversy, strength, and weakness of the test methods, theory, findings, and applications.

The advocates of assigning projective test training primarily to the internship facility will find solace in Shakow's diagnostic-therapeutic approaches because of their service character. This aspect of the training program is self-explanatory. The student is presented with practical learning opportunities—learning that is best based on previously acquired information and skills. Moreover, the intern in most instances is a graduate predoctoral student responsible for carrying on dissertational research. He must look beyond the service function as he works with people and tests to the research referents that abound in the clinic. Much of this opportunity for growth in research interest and competency may be lost to the trainee who is absorbed in learning elementary testing techniques. With these skills already part of his apperceptive mass, he is freer to take advantage of the internship as something more than a time of learning new technical proficiencies. It becomes an added opportunity for thinking about tests, data, people, and research ideas as possible sources for supporting or modifying concepts.

In another report, a committee under the chairmanship of Shakow (6) recommended as part of the second-year graduate training program in clinical psychology a course or courses in the theory and practice of projective devices. The formal work should consist of lectures and some type of clinical practice. These would be followed by supervision in the administration, interpretation, and reporting of test results. The committee also felt that part of this introductory work could be accomplished in a clerkship setting. This recommendation lent itself to a variety of interpretations and many modes of implementation. One view would establish a course in which the student concerns himself with the rationale of the projective procedure, the issues involved, and the experimental literature. The Rorschach Test could serve as the prototype projective method with emphasis on administration and scoring in the context of the dynamics of the location, determinant, and content elements. To present these concepts the teaching would involve a discussion of personality theory, motivation, perception, and introductory psychopathology. Classroom lectures, laboratory demonstrations, and supervised practice should include individual and class critiques. This procedure permits exposing a larger number of students to a greater variety of situations in much less time than if each intern were to be given individual instruction by a single supervisor. It is important for the trainee to become acquainted with diverse problems of administration, scoring, and interpretation rather quickly. It is doubtful that an individual clinic supervisor could present for an intern's consideration during any one supervisory hour nearly as many issues as a classroom instructor could introduce in the same period to a number of students. From a practical point of view time is on the side of university preparation for the practicum.

In a discussion of the graduate professional program, the APA Committee on Training in Clinical Psychology (1) suggests a broad curriculum with no attempt to turn out a specialist. Of the six recommended major areas of study, two, psychodynamics of behavior and diagnostic methods, are deemed vital to a clinical psychology training program. Furthermore, "The program should concern itself mainly with basic courses and principles rather than multiply courses in techniques" (p. 543). The interpretation of "multiply" and "techniques" are not given. Instead, these terms are wisely left to each department for definition as it plans its own program. The variety of training proposals is testimony of the differences in interpretation extent today.

In a further analysis of expert opinion, the report of the 1950 Boulder Conference states: "The university department has the responsibility of teaching the administration, scoring, and elementary levels of interpretation of some tests for clinical use and of certifying to the clinical practicum agency the student's readiness for clinical practium use of such tests" (4, p. 115).

Finally, the 1950 report of the APA Committee on Training in Clinical Psychology (2) recognizes that there may be a lack of integration between academic and theoretical training on the one hand, and the practical experience received in the field agency on the other. The report elaborates on the issue (p. 598):

> Because it is only in the practicum that the student can develop proficiency in the use of psychological techniques, the question is sometimes raised as to the desirability of giving the initial laboratory level of instruction in techniques in the practicum agency. It is sometimes contended that early contact with patients and clinical problems gives greater motivation to learning techniques, that they may be learned

> better in practice with patients in the practicum agency under the instruction of psychologists who themselves are engaged in professional service work, rather than in the academic setting.
>
> This problem really resolves itself to the question of how, when, and where instruction in testing techniques can best be given without inroads upon the best theoretical and practical training which may be given by the graduate department and the practicum centers in collaboration. The Boulder Conference was in general agreement that "the university department has the responsibility of teaching the administration, scoring, and elementary levels of interpretation of some tests for clinical use, and of certifying to the practicum agency the student's readiness for clinical practicum use of such tests." The committee continues to see no reason for changing this recommendation concerning the teaching of testing techniques at the university level. Because of individual unique circumstances, the committee suggested that the university, perhaps after consultation with its practicum supervisors, should decide for itself when, and under whom, and where clinical techniques may best be learned.

These somewhat open-ended recommendations, stemming from the broader view of clinical training, imply that there cannot be only one road to clinical training to the total exclusion of any other approach. The committee has wisely foreseen, or perhaps it was divided within itself, that valid differences of opinion in training programs will continue to exist. The members, therefore, have determined to ensure the student of the best training available in the variety of circumstances.

Once more the question arises—the place of the projective techniques in the university curriculum. The Kass Committee on Training of the Society for Projective Techniques (3) points out that "All major projective techniques are taught in all the (APA approved university—author) programs." This refers to the 30 departments of the 45 contacted which responded to the committee's questionnaire. The committee reports that more than half of the responding departments taught the Rorschach Test in connection with "courses on projective techniques in general or in some other context. Courses exclusively on the thematic techniques are offered in 30 per cent of the programs" (p. 121). Offering formal university courses and seminars in projective techniques enables the student to spend classroom, laboratory, and library time learning the rationale of the projective method, reading the pertinent research with regard to the problems and issues involved in the validity and reliability of the procedures, and integrating test theory and application. The usual interchange between student and instructor during lecture and laboratory sessions is feasible. Formal semester reports and frequent papers on current research provide essential referents. Much that can be exchanged in the atmosphere of the classroom, with stimulation to both student and teacher, may be lost in the busy clinic where service-demands can effectively reduce the time spent in teaching by the supervisor. The imparting of projective skills is a dual responsibility which neither the lecture hall nor the clinical faculty can do alone efficiently.

The aim of the internship is to furnish experiences with people so that the student may apply knowledge gained in the classroom. But the intern must come to the practicum with basic information and skill. Elementary competence with projective devices, a theoretical point of view, an understanding of behavioral dynamics and how these are rooted in perceptual psychology enhance the value of the practicum. It removes from the agency the responsibility of conducting clinical training "from the ground up." The reply that "We want to train the intern our way" implies that "our way" is *the* right way and that other modes are somehow wanting. If clinical psychologists were dealing with a subject matter other than human behavior this argument might be tenable. But this discipline is not ready, as yet, to admit of only one way to assess human behavior for the purpose of yielding inferences and for making predictions therefrom. It would be unwise to send an intern to the practicum agency without prior contact with the tests to be used. It is less frightening to the trainee and gives the training supervisor some assurance of the "student's readiness for clinical practicum use of such tests." Projective testing courses in the university do not usurp the role of the practicum supervisor. The latter completes the work of the former, which do belong there.

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EXPRESSED INTERESTS IN LATER CHILDHOOD*

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A. INTRODUCTION

Writings dealing with the origin and development of interests are few in number. Fryer's (6) volume published nearly 30 years ago, summarizes the research of the preceding decade, indicating that during that decade interests achieved the status of a separate entity,—separate from emotion, motivation, ability.

In his review of the research literature some 15 years later (1944) Berdie (2) states that both interests and abilities are co-determiners of achievement. Furthermore, he maintains that as interests are more completely understood, psychologists will better understand many unsolved riddles of personality.

Strong (11) maintains that human behavior involves wants or desires which are to be satisfied by reaching some goal. A little later in the same volume, he maintains that interests are learned and may accordingly be modified later on by re-education.

After discussing various studies on the origin and development of interests, Darley and Hagenah (5) state that "running through much of what has been written about the origin and development of interests is the question of what people want in life." In Roe's (8, 9, 10) careful research, one recalls that her scientists were highly motivated by interests intrinsic to their work.

Probably one of the best studies of the age at development of interests is that presently under way by Tyler (12), a portion of which has already been reported in the literature. Using a paired comparison interest inventory with 115 first grade children, administered orally, Tyler tapped four kinds of interests: outdoor play, indoor play, paper-pencil-crayon activities, and helping adults with work.

Allport (1), in his book on *Personality* in a chapter entitled "The Transformation of Motives," states that in the process of maturing, the manifold potentialities and dispositions of childhood coalesce into sharper, more distinctive motivational systems. He refers to evidence of a marked relation between ability and interest, stating that the skill learned for some external

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reason becomes an interest, and then is self-propelling, even though the original reason for pursuing it has been lost.

While much has been revealed in these and other studies of interest, there is need for basic research in the freely expressed interests of children and youth.

B. Method of the Study

In order to study the true interests of nine-year-olds one must get at the things that are important to children at this age level. The writer feels that checking items on questionnaires and/or inventories involves inherently a subjective element. There is at least the element of suggestion present. On the other hand, when children are permitted to state their interests freely and without any pre-discussion on the matter, one might expect the results to be more accurate so far as their real, true interests are concerned. Through a deeper knowledge of the interests that children consider vital to themselves or to others, one may hope to come to a better understanding of children. Such is the purpose of the present study.

1. *Subjects*

For the present study of children in the later childhood period it was decided to use a sample of fourth grade boys and girls from widely separated geographical areas of the United States. A number of schools was contacted on the east coast, on the west coast, and in the northern, southern, and central states. Ten schools, one each in North Carolina, Maine, Pennsylvania, Minnesota, Texas, Missouri, Louisiana, Indiana, California, and Washington, cooperated. All were private schools. City or town in which schools were located ranged in population from 2000 to 800,000. School population ranged in size from 132 pupils to 948 pupils.

2. *Procedure*

After securing the cooperation of principals and teachers, the following information was sent to the participating schools:

> This is a research study of the interests of boys and girls in later childhood in various parts of the United States. We appreciate your kind cooperation in our research. Read the directions to the children; then give them time to write the answer to the question. Encourage the slower children to write faster. Nothing is gained by prolonged thought on a topic such as interests.
>
> You are especially asked not to discuss the topic of interests with pupils before they write their answers. Also kindly see to it that the children do not communicate with one another during the time they are writing.

> *Directions to the children:* "On an ordinary sheet of tablet paper, write your name, sex, grade, age, date, school, city, and state. When that is completed I will read to you the question you are to answer. You will make three choices. Number them, one, two, and three respectively."

After all children have completed recording the data at the head of the page, explain as follows and then ask the question.

> You have been asked by the Head of the Department of Psychology of a college in the Midwest to cooperate in a study of the interests of children. Your principal and teachers here would like for you to participate in this research project. It will take but a few minutes for you to answer the question.
>
> Now, I want you to think for just a moment of what are your greatest interests at this time. It matters not what it is. Just think of your greatest interest or desire at this time. The question is: "What are three of your greatest interests, in order of importance to you?" In other words, if these three interests could be fulfilled, what would you most desire? List your first choice as number one, your second choice as number two, your third choice as number three.
>
> As soon as you have completed the work, I will collect your papers, put them in this envelope, and mail them to the psychologist who is conducting this research study.

C. Preliminary Analysis

Preliminary inspection of the papers received from the 10 cooperating schools revealed that classes ranged in size from 18 to 52 pupils, with an average class size of 41 pupils. Because of too few schools in each area, no attempt was made to analyze or investigate possible differences according to geographical location.

From the fourth grade classes in the 10 schools a total of 409 papers was received. Of these, 27 papers were eliminated for one or more of three causes: illegible handwriting, ambiguity of response, incomplete data at the head of the paper. This left a total of 382 papers for the study.

1. *Age Analysis*

The study included 178 fourth grade boys ranging in age from 8 to 12 years, with a mean age of 9.3 ± .05. The 204 girls' papers indicated an age range from 8 to 11 years, with a mean age of 9.2 ± .03.

2. *Interest Categories*

Preliminary to the analysis of the data it was necessary to tabulate all individual items separately for boys and for girls. It soon became apparent that these items grouped themselves under surprisingly few headings. Using code numbers both for each individual pupil and also for the designation of his first, second, and third interest, charts were constructed for boys and for girls separately with nine resultant groupings. These were: possession or ownership of objects, good life, pets, vocation, education, travel, relatives, money, and school. A tenth chart was added, marked "miscellaneous." This contained the many different items, each of which was encountered but a few times.

3. *Analysis of Interest Groupings*

When children are free to express their interests without any prior suggestions or presentation of items on prepared lists or inventories, one might expect to find nearly as many items as there are children in the study. However, this was not the case in the present investigation. Between 85 and 90 per cent of all interests could be grouped under the nine headings above mentioned.

For a study of this nature, percentages were deemed more meaningful than other possible methods of study. Blanc (3) also found percentages more meaningful in the interpretation of interests. He states: "To obtain a more accurate interpretation, it was decided to employ the percentages of the total cases in each category of grouping."

TABLE 1

ANALYSES OF FREELY EXPRESSED INTERESTS OF 382 FOURTH GRADE BOYS AND GIRLS

Interest category	First interest		Second interest		Third interest	
	Boys	Girls	Boys	Girls	Boys	Girls
Things owned	35.9	30.4	42.7	36.2	32.6	27.4
Good life	16.9	17.4	14.6	9.8	11.2	10.8
Pets	10.1	13.7	12.4	14.7	10.1	16.6
Vocation	9.1	3.9	10.1	7.8	9.2	13.7
Travel	5.6	6.8	4.5	5.9	7.8	7.8
Relatives	3.4	7.8	2.2	3.1	5.6	5.9
Money	2.2	5.9	1.1	3.9	2.2	3.1
School	4.5	2.9	3.4	2.9	1.1	3.9
Education	1.1	1.9	1.4	1.0	3.4	2.0
Miscellaneous	11.2	9.1	7.6	14.7	16.8	8.8
TOTAL	100.0	100.0	100.0	100.0	100.0	100.0

In this preliminary analysis, data for all categories are reported separately for first, for second, and for third choices of interests. This method enables one to perceive readily the various strengths of the different categories of interests. These are presented in Table 1. It will be noted from this table that four interest categories account for nearly three-fourths of all the interests expressed by these children.

a. First choice of interests. In Table 1, the first two columns of figures reveal the first choice of the children's interests. This might be interpreted as being their greatest interests, the things that seem most important to them.

From these two columns it is evident that, by and large, nine-year-olds are interested in the possession of objects: 35.9 per cent of the boys and 30.4 per cent of the girls listed some item of ownership or possession as their greatest interest. If one combines Items 1 and 3, things owned and pets, one finds 46 per cent of the boys and 44 per cent of the girls indicating this as their first choice of interest. If one would add to that the percentages of children indicating money as their first interest, one readily sees that approximately half of the nine-year-olds are interested in possessions.

Following this one finds a decided drop, in that 16.9 per cent of the boys indicate items relative to a good life as their first choice of interest, while 14.7 per cent of the girls indicate pets as their first choice of interest. Noting the third highest frequency, one finds 10.1 per cent of the boys interested in pets, whereas 9.8 per cent of the girls indicate items relative to a good life.

Next in line of frequency of occurrence appears items relative to vocation. In this category one finds 9.1 per cent of the boys and 3.9 per cent of the girls expressing as their greatest interest, their first choice of interest, an item relative to their future vocation in life. Considerable sex differences are indicated in this item, as more than twice as many boys as girls have indicated as their first interest an item pertinent to their future vocation in life.

Travel and interest in relatives appear next in order of frequency among the boys, with 5.6 per cent listing the former and 3.4 per cent listing the latter as their first choices. In both these items one finds greater sex differences, inasmuch as 6.8 per cent of the girls are interested in travel, while 7.8 per cent listed items pertaining to relatives.

In the three remaining items, namely, money, school, and education, one finds fewer listings. Sex differences again appear, as more than twice as many girls as boys are interested in money whereas, on the other hand, nearly twice as many boys as girls are interested in some item pertaining to school life.

b. Second choice of interest. A study of Columns 3 and 4 of Table 1 indicate at a glance that relative positions of the varied interests are not greatly different from those in Columns 1 and 2.

Still larger percentages of children, 42.7 per cent of the boys and 36.2 per cent of the girls, are interested in the possession of objects. Second choices of interests indicate a greater interest in pets than did their first choice of interest in the case of both boys and girls. Interest in a good life has decreased as a second choice of interest.

In the next item, vocation, one finds larger percentages, 9.1 per cent of the boys and 7.8 per cent of the girls, indicating this item as their second choice of interest. For the boys this increase is slight whereas for the girls it is nearly double that expressed in the first choice of that interest. Nearly as many expressed a second choice of interest in travel as did those who expressed this item as their first choice. The decrease was slight, approximately 1 per cent.

Interest in items pertinent to relatives also experienced a decrease in number of children expressing this as second choice. For the girls, less than half as many expressed this as a second choice, as did those expressing it as a first choice. The remaining interests also found fewer children listing them in their second choice of interest.

c. Third choice of interest. A study of the fifth and sixth columns of Table 1 indicating the children's third choices of interest revealed that possession of objects continues to hold first place. Other interests were reported with frequencies in approximately the same order as heretofore.

Two items worth noting concern pets and vocation. While approximately the same number of boys indicate these two items in first, second, and third choices of interest, one notes a steadily increasing number of girls choose these items. For girls, pets were listed as a first choice, 13.7 per cent; second choice, 14.7 per cent; and third choice, 16.6 per cent; which means that more girls are listing other things as their first and second choices.

The same holds true with regard to items pertinent to vocation. In the case of the boys one finds approximately the same number of boys listing an item of vocation in their first choice, second choice, and third choice; whereas in the case of the girls, very few indicated this as their first choice, a larger number as a second choice, and a still larger number as a third choice, the figures being 3.9 per cent, 7.8 per cent, and 13.7 per cent, respectively. Collectively, this is a large percentage of nine-year-old girls thinking in terms of a future vocation, nearly one-fourth of the girls in the study. However, other interests come first as indicated by the extremely small per cent listing this as their first choice.

4. *Composite Values of Interest Category*

As nine categories of interests emerged, it was deemed advisable to study their relative strengths. Hence, all listings in each category were combined according to their relative strength as first, second, and third choices of expressed interest. In this way, each item listed by a pupil exerts its proportionate rank. The resultant weighted scores can then be compared directly.

Following Strong's(11) modification of weights devised by Cowdery (4) and Kelley (7), the writer weighted the items by applying weights of +1, +2, and +3 respectively, to the third, the second, and the first choices of interest. This composite indication of frequency of interests among the 382 children in the present study is presented in Table 2.

TABLE 2
COMPOSITE SCORES FOR EACH INTEREST CATEGORY FOR 382 BOYS AND GIRLS

Interest category	Rank	Boys	Girls	Sexes combined
Things owned	1	9.9	9.7	9.8
Good life	2	2.8	3.9	3.4
Pets	3	1.6	4.3	3.0
Vocation	4	2.1	1.9	2.0
Travel	5	1.2	1.7	1.5
Relatives	6	0.6	1.5	1.1
Money	7	0.5	1.4	0.9
School	8	0.8	0.6	0.7
Education	9	0.1	0.1	0.1
Miscellaneous		1.5	3.6	2.5

Inspection of the last column of this table reveals clearly wherein lay the greatest interests of nine-year-olds. The largest score, 9.8, for the possession of objects is nearly three times greater than the second ranking interest, good life, whose weighted score for sexes combined is 3.4. Pets and vocation come next with scores of 3.0 and 2.0, followed by travel with a score of 1.5, and interest in relatives with a score of 1.1.

Sex differences and similarities in interest are apparent in the weighted scores. Interest in the possession of objects is practically the same for both sexes. Interest in pets is nearly three times as great among girls as it is among boys. Interest in items pertinent to a good life claim more girls than boys with scores of 3.9 and 2.8 respectively. Sex differences are also notable in interest in relatives. Here one finds nearly three times the amount of interest among girls as among boys. The same holds true for interest in money.

D. SPECIFIC CATEGORY ANALYSES

Because a large percentage of both boys and girls expressed as their first interest the possession of some object, another large percentage expressed this as their second choice of interest, and a third large percentage expressed it as their third choice of interest, a detailed analysis of the various objects, the possession of which claimed great importance for these nine-year-old boys and girls was deemed advisable. Other categories of interest will be analyzed in less detail.

1. *Interest in Possession of Objects*

An analysis of the items of interest reported by the boys and girls of this study relative to the possession of objects is presented in Table 3. In this table the combined responses of first choice, second choice, and third choice were used. In order to get an accurate picture of the percentages of boys and girls interested in the possession of the individual objects, it was deemed more appropriate to use the unweighted scores. The 100 per cent listed as the total at the end of each column represents the total of all objects.

TABLE 3
ANALYSES OF INTERESTS IN THE POSSESSION OF OBJECTS

Things to own	Boys	Girls
Animals	1.7	0.0
Athletic equipment	8.7	0.0
Balls	5.2	4.9
Boats	5.2	1.9
Books	1.5	5.8
Bikes	20.8	24.5
Cameras	2.1	2.5
Cars	11.3	1.9
Clothes	0.9	7.8
Food	2.6	0.5
Dolls	0.0	10.1
Games	1.2	2.3
Guns	6.8	0.0
House	4.3	6.8
Knives	0.8	0.0
Piano	0.6	3.9
Planes	2.8	1.2
Playhouse	0.0	4.5
Religious articles	1.7	3.1
Skates	0.9	2.1
Sleds	1.5	1.7
Swimming pool	0.4	1.5
Television & radio	3.6	2.3
Trains	2.3	0.0
Wagons	1.9	0.9
Watch	0.8	2.9
Miscellaneous	10.4	6.9
TOTAL	100.0	100.0

A glance at Table 3 reveals that the possession of a bicycle heads the list both for boys and for girls, with 20.8 per cent and 24.5 per cent, respectively. Next highest for boys is the possession of a car, 11.3 per cent; while for girls it is interest in dolls, 10.1 per cent.

If one glances down the table at the various objects providing for locomotion, one notes that the figures for bikes, cars, boats, planes, and trains yield a combined percentage of 42.4 for the boys and 29.5 for the girls. Earlier it was noted that the largest percentage of interest was for the possession of objects. Now one finds that nearly half of these objects for boys and close to one-third of the objects for girls involve some means of locomotion. Could that be indicative of the restlessness of these boys and girls, of their anxiety to go places, of their wanting to get away, of their longing for adventure? While this study does not investigate causes, it does indicate something worth further research.

A number of sex differences are also revealed in this table. More girls than boys were interested in bicycles whereas more boys than girls wanted cars. Girls showed decided interest in such items as dolls, clothes, books, playhouse, piano, religious articles, and skates. Boys, on the other hand, showed a greater interest than did girls in such items as balls, athletic equipment, guns, boats, trains, television and radio, wagons, food, animals and knives.

Other items reported too infrequently to warrant separate listing were grouped under "miscellaneous." These include such things as bow-and-arrow, saddle, rings, keys, dishes, musical instruments, doll clothes, a big back yard to play in, skating rink, a chemistry set, golf clubs, pool table, and flashlight.

From this analysis, it is apparent that after objects involving some means of travel or getting places, most of the other items could be grouped into various types of play materials.

2. *Interest in Good Life*

A number of items listed both by boys and by girls pertain in one way or another, to elements best categorized as "good life." The items tabulated under this heading referred generally to goodness, the practice of virtue, usefulness to others, good moral character, and the like.

It will be noted that items pertinent to a good life ranked second highest among boys in first choice, second choice, and third choice. Among girls it ranked second highest in first choice of interest, while it dropped to third place among second choices, and fourth highest among third choices. Yet, when all choices are considered, approximately one-eighth of the items listed by these boys and girls pertain in one way or another to good life.

Many of the items listed by the children pertain to *living* a good life, but by far the larger number seems to have reference to the ultimate purpose of life. In other words, the boys and girls who listed these items are looking toward the outcome of a good life, eternal happiness.

While a larger percentage of girls listed this as their first choice of interest than did boys, larger percentages of boys listed it as their second choice than did girls. The same holds true for the third choice of interest.

Specific items listed under this category include such as: to lead a good life, to help others to be good, to be good, to do things for others, to go to heaven, to be good every day, to be in heaven, to be a good Catholic, to be helpful to others, to have a happy life and death, to help others return to God, to see God in heaven, to prepare for heaven, to be a good citizen, to serve God, to gain eternal happiness. Other items specified such interest as to receive the sacraments, to pray the rosary every day, to be a saint, to get grace, to see Jesus in heaven, to pray daily, to keep the faith, or other definite reference to religious belief or practice.

Weighted scores of 2.8 for boys and 3.9 for girls indicate that more girls than boys are interested in items pertinent to a good life. Although approximately the same number of boys listed this item as first choice, second choice, and third choice, it will be remembered that by far the larger percentage of girls listed it as their first choice. However, in the combined weighted scores, good life with a score of 3.4 achieved the second highest among the nine categories. This would seem to indicate that even nine-year-old children can be interested in things pertaining to a good life. Knowledge of this is important to all persons dealing with children. Perhaps if more children at an early age could be directed to the development of interests in things pertaining to a good life and to one's ultimate goal, eternal happiness in heaven, many more young people might be prevented from becoming delinquent.

3. *Interest in Pets*

Interest in pets ranked third among boys, with 10.1 per cent choosing it as their first interest, 12.4 per cent as their second interest, and 10.1 per cent as their third interest. Sex differences are apparent when one compares the above figures with those for the girls' choices. Here one finds more girls indicating pets as their first choice than boys, a total of 13.7 per cent as against 10.1 per cent. Pets still hold third place in rank among girls' first choice of interest; however, an increasingly larger percentage of girls indicate it as their second choice of interest, 14.7 per cent; and a still larger number of girls indicate it as their third choice of interest, 16.6 per cent.

Reference to the weighted scores wherein choices are represented proportionately, one finds sex differences still more apparent. Among boys, pets received a weighted score of 1.6 with a rank of fourth place; among girls, the weighted score is 4.3, the second highest-ranking interest. When weighted scores for sexes are combined, that for pets is 3.0, ranking third among the nine interest categories.

Among the specific items listed under pets, one finds the dog listed most frequently, followed by the pony, ranking second. Other pets holding the child's interest include: cats, birds, goldfish, horse, pet sheep, ducks, puppies, lamb, cow, calf. Several indicated their interest in having a pet. Apparently these had none.

4. *Interest in Vocation*

It may come as a surprise to some to find children in the nine-year-age group indicating vocation when permitted a free expression of their interests. Yet, since many boys and girls listed items under this heading, it was deemed advisable to make a separate analysis of the various items. It will be remembered that more than 9 per cent of the boys wrote this as their first choice, 10 per cent as their second choice, and another 9 per cent as third choice. Approximately the same number of girls indicated vocation as their interest although the pattern of degree of choice was quite different from that of the boys. Nearly 4 per cent of the girls indicated this as their first choice of interest, nearly 8 per cent as their second choice, and another nearly 14 per cent as their third choice of interest. Evidently more girls have other interests more pressing as indicated by first and second choices, yet the large increase in third choices indicates that this interest is not far from their minds. Among weighted scores, vocation ranks fourth among the nine interest categories. It is surpassed only by interest in the possession of objects, good life, and pets.

The various responses of the boys and girls with regard to expressed interest relative to vocation are presented in Table 4. It will be noted from this table that a wide variety of vocational choices are represented among the interests of these children. Boys are interested in such things as getting a good job or service in the armed forces, represented by 17.9 per cent and 14.9 per cent respectively. Another 22.5 per cent are thinking of a vocation as a priest. Next in line of rank is interest in marriage, 8.0 per cent. Other vocations include scientist, doctor, teacher, airplane pilot. Smaller percentages listed television star, engineer, dancer, policeman, circus performer, religious brother, printer, musician, and farmer.

Analysis of girls' interest in vocation reveals that the largest percentages are looking forward to becoming a nurse or a nun, 18.4 per cent and 21.5 per cent, respectively. Another 12.3 per cent is looking toward marriage while 11.7 per cent look forward to becoming teachers. The last two named indicate considerable sex differences, inasmuch as more than three times as many girls as boys listed teacher; while nearly 50 per cent more girls than boys listed marriage. Other vocations include secretary, movie star, doctor, circus performer, dancer, singer, artist, writer, musician, and television star. It is interesting to note that both among the boys and among the girls one finds a small percentage listing as one of their interests, knowing their right vocation or finding their right vocation, as indicated by 3.2 per cent of the boys and 4.6 per cent of the girls. This could possibly indicate a greater degree of maturity among these few boys and girls who are deeply interested in vocation and are at the same time aware of the necessity of finding their right vocation in life.

TABLE 4
ANALYSES OF INTEREST IN VOCATION

Vocational interest	Boys	Girls
Armed forces	14.9	0.0
Artist	0.0	2.8
Brother	2.8	0.0
Circus	0.8	0.6
Cowboy	0.5	0.0
Dancer	1.5	1.2
Doctor	3.5	1.8
Engineer	2.7	0.0
Farmer	1.2	0.0
Good job	17.9	7.3
Marriage	8.0	12.3
Musician	1.1	1.8
Movie star	2.8	2.4
Nun	0.0	21.5
Nurse	0.0	18.4
Pilot	3.1	0.0
Policeman	1.3	0.0
Priest	22.5	0.0
Printer	0.6	0.0
Right vocation	3.2	4.6
Scientist	4.3	0.0
Secretary	0.0	5.8
Singer	0.0	3.2
Teacher	3.2	11.7
Television star	2.3	2.5
Writer	1.8	2.1
TOTAL	100.0	100.0

5. *Interest in Travel*

With many youngsters interested in planes, trains, and cars, one is not surprised to find at least a small percentage of children listing some form of travel as their chief interest.

While interest in travel is not as great as that in the four above mentioned categories, yet it did claim first choice among 5.6 per cent of the boys and 6.8 per cent of the girls. Percentages were approximately the same among those who indicated it as their second or their third choice. No great sex differences are apparent in this category.

Specific items listed under this category include: to go overseas, to go to South America, to go to Hawaii, to go to China, a trip around the world, to cross the Atlantic, to cross the ocean, a trip to the lake, a trip to a planet, to go to New York, to go to the country, to go to Montana, to go to Italy, to go on a trip, to travel all over the world, to go to Florida, to Seattle, to Washington, to Chicago. However, in this day of space ships and talk of interplanetary travel one is not surprised to find a number of children expressing this as their greatest interest.

6. *Interest in Relatives*

When children are asked to express their greatest interest, it would seem unlikely that that greatest interest would refer to someone other than themselves. Yet the present study indicated that at least a few of them were greatly concerned either with a member of their own family or with someone among their relatives.

This is the category of interest in which sex differences are most apparent. More than twice as many girls as boys listed an item under this category as first interest, 7.8 per cent of the girls and 3.4 per cent of the boys. In their second choice of interest, 3.1 per cent of the girls and 2.2 per cent of the boys list this item while in their third choice of interest one finds about the same number listing this item in each of the sexes, 5.9 per cent of the girls and 5.6 per cent of the boys. However, in the weighted scores for this category one finds a score of 0.6 for the boys and 1.5 for the girls. This indicates that the strength of this interest among girls is nearly three times as great as among boys. The combined weighted scores, 1.1 for this category, places it fifth among the nine categories of interest.

Among specific items listed under this heading one finds no sex differences apparent. Items included: that my uncle would get well, that my brother would get home from service, that my sister could hear again, that my aunt would get well, to have a little sister, conversion of my father, a new dishwasher for mother, conversion of family, to have father home with us, that my uncle's family be converted, that mother would be able to go to Christmas Mass, that mother would get well, that my aunt would go to church again, conversion of grandma, that my uncle come back to church.

Ordinarily one would not think that nine-year-old children could be so concerned over family matters. Yet this study did indicate such concern inasmuch as these items were listed by approximately 5 per cent of the population studied.

7. *Interest in Money*

In this materialistic and secularistic age, one is not surprised to find even among young children an interest in money. It is surprising, however, that a much larger percentage have not listed money as their chief interest.

It also comes as a surprise that many more girls than boys listed money as their interest. Among the boys one finds 2.2 per cent listing it as their first interest, 1.1 per cent as their second interest, and 2.2 per cent their third choice of interest. Among the girls, 5.9 per cent indicated money as their first choice of interest, while 3.9 indicated it as their second choice, and 3.1 per cent as their third choice of interest. Weighted scores for this item are 0.5 for boys and 1.4 for girls. Again, one finds the strength of this interest among nine-year-olds almost three times as great for girls as for boys. When sexes are combined for weighted scores, money ranks seventh among the nine categories of interests.

Examination of the specific items listed under money indicate these to be of two kinds, those expressing the mere possession of money and those expressing a purpose for the money. The first include such items as to have a hundred dollars, to have a thousand dollars, a bigger allowance, or to have money. Those expressing a purpose for the money included such as to have twenty thousand dollars for a new home, to have enough money to buy an airplane, to own a large business, and the like.

8. *Interest in School and in Education*

Items listed under school and those listed under education were tabulated separately because those pertaining to the former included such as indicated concern with the child's present school life, while those under the latter were concerned with the child's education generally, mostly higher education.

A larger number of boys than of girls expressed an item relative to school life as their first choice of interest, this being 4.5 per cent of the boys and 2.9 per cent of the girls. Listing it as their second choice of interest were 3.4 per cent of the boys and 2.9 per cent of the girls. In their third choice of interest, one finds still fewer boys, 1.1 per cent, and a larger number of girls, 3.9 per cent, indicating school life as their choice of interest.

Most items listed under the category of school indicated an interest in better achievement. Such items as, to be smart in school, to be good in spelling, to be good in school, to get better grades, and the like. Interestingly enough, one child listed "no school." A few others wished they were out of school.

Sex differences disappear in the last category, education. Weighted scores in this category were 1.1 for boys and 1.1 for girls. Items listed here were for the most part general, including such as to go to college, to get a good education, to get college degree. A few listed their interest in going to a specific university.

9. *Miscellaneous Interest*

The above nine categories of interest account for nearly nine-tenths of the expressed interests of the children in this study. The remaining items, too few to list under separate categories, included: to have more friends, to grow up to be a fine lady, to belong to the girl scouts, to be a baby sitter, to read books on nature study, to live on a farm, to live in the city, to have pretty hair, that we'd get a new bedroom set, that I could fly, to be king, to become a Catholic, to stop the cold war, to be May queen, to play an accordion, to be a better swimmer, that we'd always have summer, to fly like a bird, to be a queen, to be 16 years old, to live in a palace, to walk in space, world peace, that I were a man already, and to have a home of my own, to see famous people, to win the prize.

E. Summary

This investigation of interests of children in the later childhood period from various sections of the country was designed to ascertain the true interests of these boys and girls by according complete freedom in the expression of their interests.

Responses for boys and for girls were tabulated separately. These resulted in nine interest categories including possession of objects, good life, pets, vocation, travel, relatives, money, school, and education. The few remaining items were listed under miscellaneous.

Data were analyzed for each sex separately for first, second, and third choices of interest. Weighted scores for each category of interest for boys, for girls, and for sexes combined were likewise computed.

Those interest categories showing the highest percentages were given separate and more detailed analysis.

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NUTRITIONAL FACTORS IN PSYCHOLOGICAL TEST BEHAVIOR*

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A. INTRODUCTION AND PROBLEM

The "Nature vs. Nurture" controversy has occupied a prominent place in psychological literature in the past few decades, and although the volume of material in periodicals has declined somewhat recently, it is still discussed at length by child psychologists because of its implications in child rearing. Few psychologists take extreme positions in the controversy at the present time, admitting that both hereditary and environmental factors influence mental development. The question of the relative importance of each has not been answered, and therefore, the controversy is still alive today.

It is the purpose of this paper to point to an environmental influence which the authors believe has received too little consideration in the past. The term "environment" is very broad, and environmental influences should theoretically include everything that has happened to the organism since its genetic constitution was determined. Psychologists usually deal with a much narrower conception of environment, however, restricting it largely to postnatal influences, and primarily to what might be called the "intellectual" or "cultural" environment. Parental education and attitudes toward education along with other socio-economic variables generally are considered to be of prime importance in studying environmental influences on mental test behavior. Other factors frequently are mentioned, but either they are assumed to make only very minor contributions, or else are believed to be so highly correlated with socio-economic standing that they do not need to be considered separate-

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ly. One of these factors, and the one with which this paper is concerned, is the nutritional status of the individuals involved. The effects of nutritional deficiencies upon intelligence, or at least upon intelligence test scores, have not been systematically investigated. The neglect of this particular factor may prove to be unfortunate. Experts in the field of nutrition have found dietary differences between socio-economic groups as well as unexpected deficiencies in middle and upper socio-economic groups all of which were sufficiently serious to affect physiological development and performance. Considering this, it certainly seems possible that nutritional deficiencies could have significant influences upon mental growth and performance as well.

The present study is a part of a group of studies aimed at evaluating both the physiological and psychological effects of nutritional deficiencies related to citrus fruit and subsequent supplementation. More specifically, this paper deals with original differences in scores on mental ability tests found between groups matched on socio-economic status, but differing in citrus product intake, and also with the effects of orange juice supplementation on these same groups.

B. Procedures

The original pool of subjects available for this study were distributed as follows: 60 subjects in the fourth, fifth, and sixth grades of a private elementary school; 136 subjects ranging from kindergarten through the sixth grade in a second elementary school; 40 subjects in the seventh, eighth, and ninth grades of a junior high school; and 115 subjects from a woman's university. The three grade schools were all in different cities several hundred miles apart. Subjects received comprehensive physical and physiological tests and examinations as well as a battery of psychological tests at the beginning of the experimental period for their particular school, although all four groups did not begin the experiment simultaneously. The mental ability tests employed in the test battery were: Kindergarten and 1st Grade, Pintner General Ability Tests: Verbal Series: Pintner-Cunningham Primary Test (6); Grades 2 and 3, Pintner General Ability Tests: Verbal Series: Pintner-Durost Elementary Test, Scale 1 (7); Grades 4, 5, and 6, Pintner General Ability Tests: Non-Language Series: Intermediate Test (8); Grades 7, 8, 9, and College, Otis Quick-Scoring Mental Ability Tests: New Edition: Gamma Test (5). Alternate forms of all tests were available for retesting. Subjects who were tested four times received one form for the first and third tests and the other form for the second and fourth tests.

To study the effect of pre-experimental citrus fruit intake on mental ability test scores, all subjects were placed in either a high or low intake group on the basis of blood plasma ascorbic acid concentration. Blood plasma ascorbic acid was determined by the method of Mindlin and Butler (4), as modified by Bessey (1). Subjects with 1.1 mg./100 ml. blood plasma or greater were assigned to what will be called the *higher range group*, while those with lower concentrations were assigned to what will be subsequently called the *lower range group*.[2] This classification has been discussed by Mack and Urbach, *et al.* (3). In brief, it is believed that concentrations of 1.1 mg./100 ml. blood plasma or greater are necessary for cellular ascorbic acid to be at an optimum level.

Original comparisons of the higher and lower range groups on socio-economic data demonstrated that the higher range group was superior to the lower range group. In order to eliminate this difference, subjects were matched on the data which were available. The variables considered and the allowable differences between matched subjects were: (*a*) Father's income within one income category. Income categories covered a $1000.00 range up to $10,000.00, and a $5000.00 range beyond. Income data were available only for the two elementary schools, and hence were not used in matching in the other two groups. (*b*) Father's education within one year. For this purpose, a high school graduate was considered to have 12 years of education; all Bachelor's Degrees were considered as indicating 16 years, Master's Degrees as indicating 17 years, etc., regardless of the actual time spent in school. (*c*) Mother's education within three years. The criterion for matching on the mother's education was less stringent because it was felt that the father's education was more indicative of the overall intellectual and social status of the family. To try further to insure comparability of background, each subject was always matched with another subject from the same school, although age and sex were not considered. However, since the only psychological tests employed in comparing the groups yielded Intelligence Quotients as the score, the age and sex variables should not be crucial.

The data for the comparison of the higher and lower range groups on the socio-economic measures are shown in Table 1. It is obvious from the number of cases shown that it was impossible to find matches for a goodly proportion of the subjects in all four schools. It also should be mentioned that the data on income are subject to some error because exact incomes were not available, and for computational purposes all incomes had to be treated as though they fell in the middle of the income category checked by the parents.

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[2] Classifying subjects as to citrus fruit or citrus fruit product intake on the basis of blood ascorbic acid concentration is not infallible. Subjects in the higher range group may have high blood plasma ascorbic acid concentrations from eating other foods rich in ascorbic acid besides citrus products, although this group of foods constitutes one of the richest sources of this nutrient in the diets of the majority of Americans. However, subjects in the lower range group undoubtedly do not have an optimum citrus product intake, as sufficient intake should keep up the concentration regardless of other food intake.

TABLE 1
COMPARISON OF HIGHER RANGE AND LOWER RANGE GROUP ON SOCIO-ECONOMIC MEASURES

		Income	Father's education in years	Mother's education in years
Higher blood plasma ascorbic acid range group	Mean	$8800.	14.2	13.0
	Mdn.	$7300.	14.2	12.9
	Standard deviation	$4893.	3.2	2.5
	N	50	72	72
Lower blood plasma ascorbic acid range group	Mean	$8790.	14.1	13.3
	Mdn.	$7429.	13.9	12.9
	Standard deviation	$4285.	3.2	2.3
	N	50	72	72

Consideration also was given to matching on fathers' occupational status, but the idea was discarded. Mere classification of some occupations as to type is difficult enough, but assignment of some relative rank with respect to the occupation's role in determining social standing or providing an environmental advantage for children is a practical impossibility. While matching occupational status by case was not attempted, a comparison of the two groups with respect to job categories was made, and these data are presented in Table 2. The method of occupational classification is described in a Department of Labor publication (2). If the job categories are dichotomized by

TABLE 2
OCCUPATIONAL STATUS OF SUBJECTS' FATHERS

Occupational classification	Higher blood plasma ascorbic acid range group	Lower blood plasma ascorbic acid range group
Professional	15	14
Semi-professional	13	10
Business (owner, manager)	18	18
Farmer	3	1
Skilled labor	15	22
Semi-skilled labor	5	5
Unskilled labor	1	0
Unemployed	1	0
Retired	1	2

placing the professional, semi-professional, and business (owner, manager) categories into one group and all others into a second group, a chi-square value of 0.47 is obtained from the resulting two by two table. Hence, a difference between the distributions of at least the size obtained could be expected to occur by chance alone about 50 per cent of the time.

C. RESULTS

The first comparison of the higher range and lower range groups was with respect to original Intelligence Quotients. The data for each school and for the total group are presented in Table 3. It can be seen that the higher range group has the higher mean in all four schools, and that the difference between the means for the combined groups attains the 5 per cent level of significance. This strongly suggests that intake of citrus fruit products is related to mental test performance.

TABLE 3
COMPARISON OF MEAN INTELLIGENCE QUOTIENTS FOR THE HIGHER RANGE AND LOWER RANGE GROUPS

	School I Kindergarten thru 6th Grade N = 37	School II Grades 4-6 N = 13	School III Grades 7-9 N = 7	College N = 15	Combined group
Higher blood plasma ascorbic acid range group	115.70	104.77	110.71	115.60	113.22
Lower blood plasma ascorbic acid range group	110.73	104.08	101.86	110.93	108.71
Significance of the difference between the means	$t = 1.74$ $p < .10$	Not significant	Not significant	Not significant	$t = 2.27$ $p < .05$

Granting that the difference found is a real difference, the question of its permanence arises. That is, can the difference between the groups be eliminated by citrus supplementation, or is the evident decrement in the performance of the lower range group an irreversible result of a probably long standing deficiency? In an attempt to answer this question, the higher and lower range groups were compared with respect to their change in scores on the mental ability tests after a period of supplementation with orange juice. The results of this comparison are shown in Table 4. It is evident that the gain exhibited by the lower range group is greater than that exhibited by the higher range group, and the difference attains the 5 per cent level of significance using a one-tailed test.[3] This result suggests that supplementation to alleviate the deficiency can effect a betterment in mental performance.

As a further check on the permanence of the original difference found between the higher and lower blood plasma ascorbic acid range groups, the mean Intelligence Quotient and mean blood plasma ascorbic acid concentration were determined for four testing periods for the two elementary schools. The plot of these means is shown in Figure 1. They have been superimposed to show the similarity. The ascorbic acid means were determined from the data for subjects who participated in all four laboratory tests. The mean Intelligence Quotients likewise were obtained employing only subjects who participated in all four psychological testing sessions, and received alternate forms of the same test in all four sessions. Since a number of subjects missed at least one test in the series, about 70 per cent of the subjects are common to both sets of computations. The means of ascorbic acid concentration fall in line with expectancy fairly well. The period between Tests 1 and 2 was a supplementation period; hence the rise in level on Test 2. The period between Tests 2 and 3 was a period with no supplementation, so the drop in level on Test 3 was again expected. Supplementation again was received between Tests 3 and 4, hence the level increase on Test 4 again was expected. No definite reason could be found as to why the Test 3 mean did not fall as low as the Test 1 mean or why the mean for Test 4 fell below the mean on Test 2. The fact that the means of the Intelligence Quotients follow the same pattern as the ascorbic acid values could not be predicted. Some increase on Test 2 could be expected because of practice effects, but the subsequent drop on Test 3, with an increase on Test 4 could not be predicted without assuming that the scores are correlated with ascorbic acid concentration.

D. Discussion

The comparison of the original test scores of the higher range and lower range groups divided on the basis of blood plasma ascorbic acid suggest that intake of citrus fruit products, as measured by blood plasma ascorbic acid concentration, has some influence on mental test performance. The remaining

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[3] Retest data were available only from the two elementary schools, as all experimental work involving these schools has been completed. Actually four separate testing sessions were held, although a number of subjects had to be dropped after Test 2 which was at the end of a school year. Testing sessions were held in December, 1955; May, 1956; November, 1956; and May, 1957. Retest data for the other two groups will eventually become available, but they will not be of value in determining the relative increase in test scores after supplementation as the subjects in this current experiment are receiving other nutrients in addition to orange juice. However, a third experiment, now in progress, is very similar to the first, and the data can eventually be used to validate some of the findings in this report.

TABLE 4
Comparison of Higher Range and Lower Range Groups with Respect to Gain in Mean Intelligence Quotient After Orange Juice Supplementation

	Mean *IQ* before supplementation	Mean *IQ* after supplementation	Mean gain	Difference between groups	Standard error of the difference between the differences	*t* value
Higher blood plasma ascorbic acid range group N = 46	112.76	112.78	.02	3.52	2.10	1.68 $p < .05$
Lower blood plasma ascorbic acid range group N = 46	109.89	113.43	3.54			

findings were that: (*a*) the lower range group seemed to show more improvement in test scores after citrus supplementation, and (*b*) the mean scores on an intelligence test closely paralleled the mean blood plasma ascorbic acid concentrations for these same periods. These results taken in combination

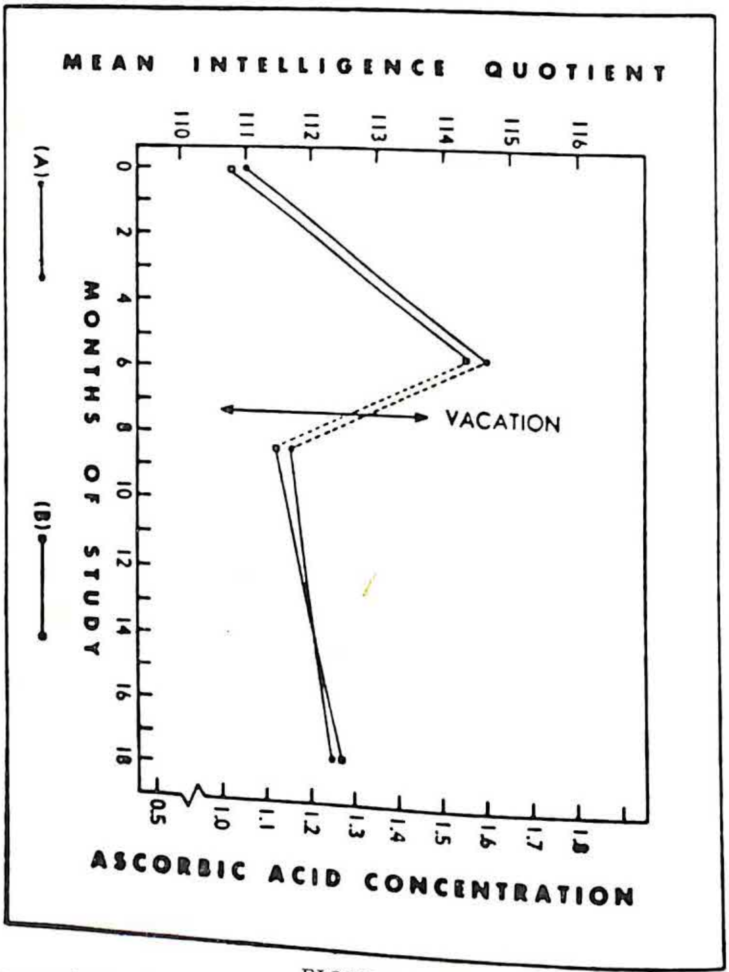

FIGURE 1

COMPARISON OF MEAN INTELLIGENCE QUOTIENT (GRAPH A) WITH MEAN BLOOD PLASMA ASCORBIC ACID (GRAPH B) FOR ALL WHO TOOK THE RESPECTIVE TESTS

indicate that some of the variance in test performance is a function of the temporary nutritional state of the individual, at least with regards to citrus or other products providing ascorbic acid. It can be hypothesized that the higher range group functioned at near maximum capacity, while the lower range group functioned at less than maximum capacity; this difference in "alertness" or "sharpness" being a function of the nutritional state. Exactly how ascorbic acid itself could increase mental efficiency can only be speculated upon. Furthermore, it must be remembered that ascorbic acid is the most obvious, but certainly not the only nutrient in orange juice. Although this investigation has centered about ascorbic acid, and it has been used as an index of citrus product intake, it is entirely possible that other nutrients found in the orange juice are actually the ones responsible for the results.

While hypotheses concerning the relationship between optimum mental functioning and ascorbic acid or any other nutrient in orange juice must remain hypotheses at the present, it can be pointed out that ascorbic acid is an ideal nutrient for investigating such hypotheses. Its storage in the body is very limited and its concentration in the blood stream changes more rapidly than any of the other major vitamins. Changes in cellular concentration are undoubtedly slower than changes in the blood plasma, but nevertheless, temporary bodily deficiencies in ascorbic acid due to temporary dietary deficiencies are probably more frequent than for any other vitamin. This being true, the effects of dietary deficiencies and eventual supplementation should become noticeable in shorter periods of time, making experimental studies more feasible.

The data presented certainly do not warrant any strong conclusions, but they do suggest that a heretofore generally ignored variable, the nutritional state of the individual, may play a significant role in determining mental test behavior even among groups where serious nutritional deficiencies are normally not expected. Naturally this experiment should be replicated and also extended to other age groups. Likewise, the effects of other nutrients should be investigated.

If the results reported herein can be substantiated by further investigations, it may necessitate a reexamination of all literature dealing with environmental effects on mental tests. The concept of environment usually employed in "Nature-Nurture" studies may have to be broadened in scope to cover other variables than just those socio-economic indices believed to be related to opportunity for mental development.

E. Summary

This study was undertaken to investigate the effect of a dietary deficiency in citrus products upon mental test performance, and to evaluate changes in performance after a period of supplementation with orange juice. Subjects from three schools ranging from Kindergarten through the ninth grade as well as a group of college women were studied.

On the basis of a pre-experimental determination of blood plasma ascorbic acid concentration, each subject was placed into either the higher range or the lower range group. Subjects in the higher range group were believed to show no indication of a dietary deficiency in ascorbic acid. Subjects within a given school system from the two groups were matched by case on a series of socio-economic indicators, and the matched groups were compared with respect to mean Intelligence Quotient on a pre-experimental test. In all four schools the mean Intelligence Quotient of the higher range group was larger than that of the lower range group, and the difference between the means of the combined higher and lower range groups was significant at the 5 per cent level.

The two groups were then compared with respect to gain in mean Intelligence Quotient after a period of supplementation with orange juice. The lower range group demonstrated a significantly larger gain than the higher range group.

Subjects from the two elementary schools were tested a total of four times during the experiment. A plot of the means showed that changes in mean ascorbic acid concentration were closely paralleled by changes in mean Intelligence Quotient.

The results indicate that nutritional factors may play a more important role in mental test behavior than has been previously assumed. The implications of these results for the "Nature versus Nurture" controversy are discussed.

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ABSOLUTE VS. RELATIONAL DISCRIMINATION ON THE DIMENSION OF NUMBER*

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A. PROBLEM

In a number of transposition studies with children as subjects, the ability to verbalize the relationship between the stimuli has been invoked in explanation for generalized transposition behavior (1, 9, 14). If this factor can facilitate relational responses in transposition tests, it should likewise work in the opposite direction, that is, to increase absolute responses, provided that the subjects have learned verbal labels to designate specific absolute values on the dimension of discrimination. An obvious case in point is the dimension of number. It should thus be of interest to study discrimination learning on this dimension, and to determine the incidence of transposition by children at varying levels of verbal development.

This, in general terms, is the purpose of the present study. More specifically, the hypothesis to be tested is that, at the lower end of the dimension of number, where the numerosity of a visually presented aggregate is readily identifiable, absolute discrimination will increase with age through the early years of primary school. The assumption on which this hypothesis rests is that the experience in the identification of number and with number concepts generally which our culture provides in this age period will increase the availability of the appropriate verbal mediator in a discrimination learning problem.

In order to provide information more sensitive than that yielded by the traditional transposition trial, the design of this study included transfer series in which either a generalized absolute response or a generalized relational response was reinforced. This approach permits one, furthermore, to examine the hypothesis both in terms of the strength of the absolute response *per se*, and in terms of the relative strengths of the absolute and the relational responses. The desirability of such a procedure is indicated by the author's finding (18), in a study applying the same design to a size discrimination problem, that the strength of the relational response is essentially unrelated to the discrimination of the absolute aspects of the stimuli.

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[1] This investigation was carried out during the author's tenure of a National Science Foundation post-doctoral fellowship at the University of Geneva, Switzerland.

Two experiments will be reported. Experiment I involves a discrimination between two stimuli, with the larger number positive during training, while Experiment II is a three-stimulus discrimination, with the middle number positive.

B. Experiment I

1. *Method*

a. Apparatus. A wooden frame fitted with two sliding doors opening sideways was mounted on a horizontal base. The whole apparatus was covered uniformly with yellow cardboard, extending at the top over the edge of the frame so as to act as a screen. Stimulus cards were hung from two hooks placed at the top of the frame, so that the cards hang directly in front of the doors. The lower edges of the cards were at a height above the base which was just sufficient to allow the subject, after he had opened one of the doors, to reach under the card without losing sight of the stimulus; in the event of a correct response he could thus retrieve a small colored chip serving as a reinforcer, which was placed into a small receptacle behind either door.

b. Stimuli. The stimulus cards were cut from pieces of heavy white cardboard, 17 x 7½ cm.; each card consisted of two 6 x 6-cm. squares, 5 cm. apart, which were joined at the top by a 1½-cm. wide strip.

Varying numbers of blue dots, 3 mm. in diameter, were drawn in the squares. In the *training series,* one side contained 5 and the other side 3 dots; a set of 10 such cards was prepared, featuring this combination of dots in a variety of configurations. Each number appeared equally often on the left and on the right sides. For the *transfer series,* two sets of six cards each were prepared. The *absolute* transfer set always contained 5 dots on one side and a number of dots either smaller (1, 2, and 3) or larger (7, 8, and 9) on the other. The *relational* transfer set contained the following pairs: 1 vs. 2, 2 vs. 3, 3 vs. 5, 4 vs. 6, 5 vs. 8, and 6 vs. 9. Finally, two *practice-trial* cards were used, showing a solid yellow semi-circle on one side, while the other side was left blank.

c. Procedure. *S* sat down in front of the apparatus, on which one of the practice-trial cards was displayed. *E* told him that he wanted to play a game with him: *E* would hide a little chip behind one of the doors and *S* was to try to find it. *E* then asked him which door he thought might be the right one, and showed him how to manipulate the door of his choice, to see whether the chip was behind it. The side with the semi-circle was reinforced. In the event of a wrong choice *S* was allowed to correct it. Following a second practice-trial with the semi-circle on the other side, *S* was told that the "moon" behind which he had found the chip would no longer be shown; instead he would from now on see some "little stars" over both doors. *E* urged him to look at them carefully, so that he would find the chip every time. This warning was occasionally repeated during the course of the learning series, if *S* gave evidence of a position habit, or of failing to look at the cards before making his choice. The non-correction method was used throughout (except for the practice trials).

The criterion of learning was eight successive correct responses. If *S* failed to reach this criterion within 70 trials, the session was terminated. For the successful *S*s a one-minute pause followed the last criterion trial, during which the *S* was shown some photographs and asked to make some perceptual judgments. *E* then told him that the game would resume, and presented either the absolute or the relational transfer cards to him, for a series of 18 trials. The first of these was a transposition trial (5 vs. 8) for all *S*s. For the absolute transfer group choices of 5 were reinforced throughout this series, while the relational transfer group was rewarded for choices of the larger number on each trial. *S*s were assigned to their condition of transfer so as to equate each group, as far as possible, on trials and errors to criterion of learning.

d. Subjects. *S*s were 48 children attending various primary schools in Geneva, Switzerland. They were divided into three age groups: Lower (*L*), Middle (*M*), and Upper (*U*). Their respective mean *CA*'s, with the associated ranges, are as follows: *L*, 5 yrs. 9 mos. (4:9 to 6:4); *M*, 7 yrs. 8 mos. (7:1 to 8:7); *U*, 11 yrs. 5 mos. (10:10 to 12:1). There were 16 *S*s in each age group. Six additional *S*s (four in Group *L* and two in Group *M*) were discarded for failing to reach the criterion of learning.

In Groups *L* and *M*, half of the *S*s were assigned to each of the two transfer conditions. In Group *U*, all *S*s were assigned to the absolute condition, as the results on the relational transfer for the other groups indicated that this task would be much too easy for the oldest group to yield any information of value.

The children came to the experiment from their classrooms, as directed by their teachers. Although little time could be spent on establishing rapport, they typically responded readily to the game element of the situation, so that attention and motivation were generally at a high level.

2. *Results*

a. Learning. The median number of trials to criterion (not including the criterion trials) was: Group *L*: 12; Group *M*: 16, and Group *U*: 0.5. The analysis of variance by ranks (indicated here, in view of the skewed pattern of these scores) yields an H of 6.75, significant at the .05 level, showing that the oldest group learned significantly faster than the other two groups.

b. Transposition. The results for the transposition trial are readily stated: all *S*s transposed, with the exception of three (out of 16) in Group *U*. (Only one of these three exceptions actually seems to reflect discrimination of the absolute number of dots of the positive stimulus, as demonstrated by the subsequent verbalizations of these *S*s and their performance on the absolute transfer series.)

c. Transfer. The results for the absolute and relational transfer series are shown in Table 1. They confirm the total absence of absolute discrimination shown in the transposition data. The three group means for the absolute series are all essentially equivalent to chance (9.0); in fact, only one *S* exceeded 12 correct responses out of 18.

TABLE 1
MEAN NUMBER CORRECT IN TRANSFER SERIES OF EXPERIMENT I

	Age groups		
	L	M	U
Absolute transfer	9.5	9.1	9.1*
Relational transfer	16.9	17.6	—

* Based on 16 *S*s; all other means based on 8 *S*s.

As for the relational transfer series, both Groups *L* and *M* followed up their 100 per cent transposition with near-perfect transfer, as indicated by the means in Table 1 (the difference between them is not significant). They thus show the kind of generalized relational response which has been found to be characteristic of children who are able to verbalize a relationship (cf. 1, 9, 13)—as almost all of these *S*s did, either spontaneously or when questioned at the end of the experiment.

The *S*s' verbalizations are in good accord with the trend of these results. Spontaneous verbalizations of the stimulus relationship during the training series were frequent at all age levels; on the other hand, the positive absolute number of the training series not only was never verbalized, but could not even be recalled by the *S*s at the end of the experiment.

C. Experiment II

This experiment follows closely the design of the previous one, but involves a discrimination among three stimuli, of which the middle number is positive during training.

1. *Method*

a. Apparatus. The apparatus, somewhat larger than that utilized in Experiment I, consisted of a wooden board mounted vertically on a base, and was again covered with yellow cardboard. The vertical face featured three windows side-by-side, covered by masonite doors opening upwards. The stimulus cards were hung directly over these doors, as in Experiment I; however, since the doors opened upwards, the part of the stimulus card suspended over any one of them was displaced into a nearly horizontal position by the action of the door as it was opened by the *S*, thus removing the stimulus momentarily from sight.

b. Stimuli. The stimulus cards, cut from white cardboard 25 x 7 cm., consisted of three 5½ x 5½-cm. squares, 4 cm. apart, again joined at the top by a 1½-cm. wide strip. The *training series* was made up of a set of nine cards, featuring 3, 5, and 8 dots arranged in varying configurations on the three squares. Each number was shown equally often at each of the three positions. For the absolute and relational *transfer series* an identical set of six cards was used, with the following number combinations: 5-8-12; 1-3-5; 2-5-9; 3-5-8; 5-9-15, and 2-3-5.

c. Procedure. The procedure was for the most part identical with that of Experiment I. Three practice trials were given, in order to show the yellow semi-circle (the positive stimulus in these trials) at each of the three positions. During the training series, 5 was positive, 3 and 8 negative. Choices of 5 continued to be reinforced in the absolute transfer series, while in the relational transfer series the middle number was correct. Both transfer series again consisted of 18 trials. The first two of these were transposition trials, one upwards (5-8-12), the other downwards (1-3-5). These were presented in counterbalanced order within each condition of transfer.

d. Subjects. *S*s were 40 children from various primary schools in Geneva. Since the discrimination task proved too difficult below the age of seven, only two age groups of 20 *S*s each were included in this experiment, corresponding to Groups *M* and *U* of Experiment I. Their mean *CA*'s and associated ranges are: *M*, 7 yrs. 10 mos. (7:0 to 8:7); *U*, 11 yrs. 8 mos. (10:8 to 12:10). There were in addition 13 *S*s who did not reach the criterion of learning, of whom 12 were in the age range of Group *M*.

2. *Results*

a. Learning. The median number of trials to criterion was 21 for Group *M* and 7 for Group *U*. This difference is significant (by the Mann-Whitney test, using the normal approximation, $z = 2.2$, $p < .05$). It may be noted that this task is considerably more difficult than that of Experiment I, especially if the number of failures to reach the criterion of learning is considered.

b. Transposition. Table 2 shows the distribution of responses on the first transposition trial, as well as the frequency of absolute responses for the two trials combined. In marked contrast to Experiment I, we find that choices on these trials are rather evenly divided between absolute and relational on the whole. In Group *M*, relational responses are still very slightly in the majority, while absolute choices predominate in Group *U*. The difference between the groups is not significant for the first trial ($\chi^2 = 4.45$, for 1 df; $p > .10$), but does reach significance when the choices on the first two trials are combined ($\chi^2 = 5.27$, for 1 df; $p < .025$).[2]

TABLE 2
DISTRIBUTION OF CHOICES ON TRANSPOSITION TRIALS IN EXPERIMENT II

	Age groups	
	M	U
First trial		
Absolute	8	13
Relational	9	7
Other	3	0
Absolute choices on 1st & 2nd trials		
Two	3	9
One	9	8
None	8	3

There was no significant difference between upward and downward transposition on the first trial: for the two age groups combined, the 5-8-12 card yielded seven relational and 12 absolute responses, whereas the 1-3-5 card produced nine responses of each kind.

c. Transfer. The results on the transfer series, presented in Table 3, bear out the greater readiness of the older *S*s to respond to the absolute values of the stimuli. The analysis of variance of these scores shows a significant interaction between age groups and transfer conditions ($F = 5.99$, $p < .05$). This interaction reflects a significant superiority of the older group on absolute transfer ($t = 2.68$, $p < .02$), in conjunction with a nonsignificant superiority of the younger group on the relational transfer ($t = 0.78$, $p > .20$). (Since considerable skewness was apparent in the distribution of scores in some of the cells, the group differences were also analyzed by a non-parametric test, with altogether equivalent results.)

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[2] Cochran's modification of the Chi-square test (2), which takes into account the order among the categories of the vertical variable (number of absolute responses) was employed here.

TABLE 3
MEAN AND MEDIAN NUMBER CORRECT IN TRANSFER SERIES OF EXPERIMENT II

	Age groups	
	M	U
Means		
Absolute transfer	9.1	15.3
Relational transfer	11.2	9.4
Medians		
Absolute transfer	8	17.5
Relational transfer	13.5	9

D. DISCUSSION

We find that the hypothesis advanced at the outset received partial confirmation from the results of Experiment II, in which the absolute response did increase with age. If this were the major finding of this study, it would clearly be essential to consider more closely the question of its generality, as well as the various factors that could have contributed to this group difference (e.g., specific instruction, vocabulary development, mental age). As a matter of fact, the findings are perhaps more remarkable for their negative aspect, i.e., the overall absence of absolute responding, in spite of the availability of the appropriate verbal mediators. In Experiment I these were completely unavailing, and even in Experiment II absolute discrimination remained very much in the background in Group *M* (cf. its median of 8.0 correct in 18 trials of absolute transfer).

This point is noteworthy, if one considers that by the age of seven or eight the children have certainly had considerable experience with elementary number concepts, both in school and out. The fact that the developmental change in absolute responding occurs well beyond this age in Experiment II, and appears to be absent altogether in Experiment I, suggests that more is involved here than the mere acquisition of the appropriate label.

As far as Experiment I is concerned, the absence of absolute responses may be attributed to the strength of the competing relational response, itself benefiting from verbal mediation for most of these children. One may point in this connection to the facility of the relational transfer, even for the youngest group, as well as to the perseveration of the relational response in the absolute transfer series (where the three groups combined made an average of 13.3 responses to the larger number).

At this point several questions arise, however. First of all, why is it that the relational response is so readily elicited in this situation, in preference to the absolute? Secondly, why does the absolute response still remain largely absent in Group *M* of Experiment II, where the competing relational response appears to be much weaker (cf. the mean on relational transfer, Table 3)? Finally, what is the reason for the discrepancy between the results for the two experiments, as regards incidence of transposition and extent of relational transfer?

Let us consider an explanation in terms of the relative strengths of the various relevant mediating responses, as they have developed in the prior history of the individual. It seems plausible that "larger" is a more familiar term, for these children, than either "five" or "middle," i.e., in the sense of frequency of usage.[3] It is also conceivable, at least, that "five" increases in strength more than "middle" in the interval between ages 8 and 11. This formulation might thus seem to account rather well for much of the results, including the consistent relational transfer in Experiment I, and the substantial reduction in this transfer in Experiment II. It fails, however, to give a satisfactory answer to the second question, relating to the poor performance of Group *M* in Experiment II on the absolute transfer series. Furthermore, even admitting that "five" is a weaker mediator than "larger," one still wonders why all but one *S* should have remained so completely impervious to the absolute hypothesis in the absolute transfer series of Experiment I, where the larger number only received 50 per cent reinforcement.

An alternative interpretation may be derived from Gestalt principles (cf. 7): in the perceptual circumstances in which the discrimination learning took place in these experiments, with the opportunity for direct comparison of the stimuli, the subject may be said to be set to respond to stimulus relationships, rather than to absolute properties of the individual stimuli. The results of Experiment I are clearly in accord with this view. But what, then, is the explanation for the lack of consistent relational responding in Experiment II? If we consider more closely the nature of the task confronting the *S* in this situation, perhaps this finding will be found not to be as inconsistent with a perceptual interpretation as it seems at first.

In order to respond to the middle-number relationship in Experiment II, the *S* must abstract it from the spatial relationships among the stimuli. Thus, when the middle-number stimulus appears at one of the end positions, the spatial dimension is in outright conflict with the dimension of variation among the stimuli, interfering therefore with the direct perception of the intermediate number relationship. This creates, then, a perceptual situation which is rather different from the two-stimulus one, where the spatial dimension provides the framework for the perception of the relationship between the stimuli; thus the cortical isomorphic gradients which have been postulated by Köhler to represent the basis for the perception of stimulus relationships (8, pp. 228ff) could not easily be invoked in the case of Experiment II.

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[3] Although comparatives are not listed separately in the Thorndike-Lorge frequency tables (16), the following figures from the Juvenile Count made by these authors (based on the content of 120 books for children aged three to eight) might be cited: "middle" has a frequency of 542, as against frequencies of over 1,000 for both "large" and "five."

It may be noted that the above interpretation does account for the lack of consistent absolute responding on the part of the younger group: in spite of the factors operating against the discrimination of the middle-number relationship, the general relational set created in this as in any simultaneous discrimination would still tend to interfere with the absolute response. The subsequent development of this response shown by the older group, however, would remain to be explained.

The present investigation clearly was not designed to test the relative merits of the two interpretations which we have considered. In any event, they are probably best treated as mutually supplementary rather than exclusive. In other words, in a discrimination problem in which verbal mediators are available to the *S*, his response will be determined by the strength which this mediator has acquired in the course of his previous experience, *as modulated* by the facility with which it is aroused in a given stimulus situation. It may be noted that according to this conception the question of absolute vs. relational discrimination is closely akin to the problem of the dominance of cues in concept formation, particularly as studied by Heidbreder (5).

In conclusion, one final implication of our study for the interpretation of transposition may be brought out. The widely discrepant results obtained in Experiments I and II indicate the need for caution in attempting to explain the transposition of an intermediate stimulus relationship in the same terms as those which have been applied to the "classical" two-stimulus case. If the analysis of the situation in Experiment II which was presented above in the discussion of the Gestaltist hypothesis is correct, it would seem that mechanisms of a somewhat higher order of complexity may have to be postulated to deal with this type of problem. In corroboration of this statement, one may point to the fact that the discrimination of an intermediate stimulus has in only very few instances (3, 15) been readily mastered and transposed by non- or preverbal *S*s; most studies, on the contrary, agree with the present one in finding this type of discrimination, and the response to intermediacy more generally, to be very difficult for animals, and for children below a certain age (4, 6, 10, 12, 17). Piaget's analysis (11) of the difficulty which young children encounter in the conceptualization of ordinal relationships points in the same direction.

E. SUMMARY

This study is concerned with discrimination learning on the dimension of number, and in particular with developmental changes in absolute responses as a function of experience with number concepts. Experiment I consisted of a two-stimulus discrimination, with varying configurations of 5 (+) and 3 (—) dots as stimuli. Tests for transposition and generalized absolute and relational transfer showed that up to the age of about 11, at least, discrimination learning remained entirely relational in these children. In Experiment II, a three-stimulus discrimination involving a response to 5 dots as against 3 and 8 dots, transposition was much less prevalent, and decreased significantly between the ages of eight and 11, while absolute transfer increased significantly.

Two alternative interpretations of the results are proposed, one in terms of the relative familiarity of the various relational and absolute mediators in the two situations, the other based on the Gestaltist hypothesis of the dominance of relational over absolute stimulus properties in perception. While the data do not permit a decision between these two interpretations, it is suggested that situational and experiential factors interact in these situations to determine the particular aspect of the stimulus to which the subject will respond.

The results of our study further indicate that the transposition of a relationship between two stimuli and that of an intermediate-stimulus relationship may represent two rather different problems, the latter probably involving processes of a more complex order.

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SOLUTION OF SIGN-DIFFERENTIATED OBJECT AND POSITIONAL DISCRIMINATIONS BY RHESUS MONKEYS*

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A. INTRODUCTION

Experimentally sophisticated monkeys can shift efficiently between sets for responding to object and positional cues when tested on mixed series of discrimination problems (4, 11). There has been no direct attempt, however, to discover if intertask interference occurs when monkeys are required to learn different types of multiple sign problems, although Harlow (3) has presented data suggesting that, if there is interference, it is rapidly overcome. The present experiment was designed to determine whether intercurrent testing on complex discriminations would generate significant intertask interference. Control groups of monkeys learned homogeneous series of sign-differentiated object ("conditional") or sign-differentiated positional ("successive") discriminations. The experimental groups were trained on both sorts of problems.

A considerable literature (1, 6, 10) has developed regarding the relative difficulty of different kinds of discrimination problems. But experimentation has been restricted largely to comparing the "successive" and "simultaneous" problems. The present experiment extends the range of relevant empirical data since another pair of problem types is compared.

B. METHOD

1. *Subjects*

Seventeen rhesus monkeys which had been trained on pattern (12), object (9), and positional discrimination (11) and discrimination reversal problems were studied.

2. *Apparatus*

The Wisconsin General Test Apparatus (4, Figure 1) and its use have been described previously.

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3. *Stimuli*

The stimulus objects were regular geometrical figures (square, cross, trapezoid, etc.) cut from 2 cm. plywood. The large and small objects had areas of 100 and 12.5 sq. cm., respectively. All of the stimuli were painted black and were presented on a white form-board containing two food-wells 12 inches apart.

4. *Procedure*

The monkeys were randomly assigned to three groups. Group 1 consisted of five *S*s which were trained on eight sign-differentiated object (SDO) discriminations. Group 2 comprised four *S*s which learned eight sign-differentiated position (SDP) discriminations. The remaining eight monkeys were assigned to Group 3, and were trained on four SDO and four SDP problems. The sequence in which the individuals of Group 3 were tested on SDO and SDP problems was counterbalanced so that half of the animals learned each pair of stimuli as an SDO, and half as an SDP problem. These subgroups were a control for possible differences in the discriminability of stimulus pairs.

The *S*'s task on the SDP problems was to choose the stimulus on, e.g., the left when confronted with one pair of identical objects (e.g., two large triangles) and to displace the stimulus on the right when presented with the other pair of identical objects (two small triangles).

On the SDO problems, *S* was required to choose one of a pair of dissimilar figures (e.g., a square) when the two objects presented were large, and to respond to the other figure (e.g., a semi-circle) when the objects were both small. The large and small pairs were interchanged irregularly from trial to trial, but each size was presented on half of the trials within each block of 24 trials. There was the additional restriction that the rewarded stimuli appear on right and left an equal number of times.

Training was continued on each problem until the criterion of 20 correct out of 24 trials was attained. Since no monkey required more than 96 trials to master any problem, training on all problems was completed in a single day. The non-correction method was used throughout the experiment, and the designation of the correct and incorrect stimuli in successive problems was made in such a way as to minimize direct positive or negative transfer.

C. RESULTS

No significant interproblem transfer was observed over the set of eight problems, and the difference between testing orders for the two subgroups of

Group 3 was insignificant. The results are therefore summarized in Table 1, which shows mean errors to criterion for the three groups over all problems of the same class.

TABLE 1
MEAN ERRORS TO CRITERION ON SIGN-DIFFERENTIATED OBJECT (SDO) AND SIGN-DIFFERENTIATED POSITIONAL (SDP) DISCRIMINATION PROBLEMS

Group	N	Mean errors SDO	Mean errors SDP
1	(5)	6.0	
2	(4)		10.3
3	(8)	4.1	7.9

These data indicate clearly that intercurrent testing on both SDO and SDP problems did not retard solution of either type of problem. Indeed, Group 3 learned both SDO and SDP problems slightly more rapidly than did the groups tested on homogeneous series of SDO or SDP problems. (Neither of the differences between Group 3 and Groups 1 and 2 is statistically significant, however.)

Six of the eight monkeys in Group 3 made more errors on SDP than on SDO problems, and the remaining two monkeys had tied error scores on the two sorts of discriminations. The binomial test indicates that significantly fewer errors were made on the SDO than on the SDP problems ($P = .016$). Comparison of Groups 1 and 2 also indicates significantly more rapid learning of SDO than SDP problems ($U = 2$, $P = .032$).

D. DISCUSSION

The results of this experiment indicate that monkeys tested on mixed series of SDO and SDP problems learn both types of problems as rapidly as animals trained on only one of these kinds of problems. This finding means that the integrity of response sets may be maintained quite as effectively when monkeys are tested on sign problems as when they are tested on simpler discriminations.

The data yielded by the present investigation also suggest that the limits of the monkey's capacity for solving SDO and SDP problems remain undefined. The traditional procedure for training monkeys to solve SDO problems has been to teach the animals the component habits one by one and then to combine the two habits in a third stage (2, 7). This procedure is apparently unnecessary with sophisticated monkeys, since the *S*s described here made fewer errors in solving the SDO problems than did Noer and Harlow's (7, p. 171) monkeys in combining two habits which had been acquired previously.

Two specific inferences regarding the classifications of discrimination problems may be made with some confidence. Any attempt at a taxonomy of discrimination tasks is likely to be incomplete if phyletic and experiential variables are overlooked. Rats may learn SDP problems rather quickly under favorable conditions, but no very rapid solution of SDO problems has been reported (5). It appears quite possible that the relative difficulty of SDO and SDP may be reversed in rat and rhesus monkey.

The monkeys studied in this experiment solved 71 per cent of the SDO problems, and 45 per cent of the SDP problems within 24 trials, the minimum number required to satisfy the criterion. Every problem was solved within 96 trials, and 99 per cent within 72 trials. Relatively naive monkeys, however, required hundreds of trials to solve similar SDO and SDP problems (2, 8). The differences in experimental methods between the present investigation and the studies cited are not so large as to render unlikely the presumption that extensive learning set tuition facilitates the solution of sign problems.

E. Summary

A group of five rhesus monkeys learned eight sign-differentiated object (SDO) discriminations, a second group of four monkeys was trained on eight sign-differentiated position (SDP) problems, and a third group of eight animals solved four SDO and four SDP problems.

The monkeys tested on both SDO and SDP problems did not make as many errors on either type of problem as did the groups trained exclusively on either SDO or SDP tasks.

Although both sorts of discriminations were solved readily, 99 per cent within 72 trials, significantly more errors were made on the SDP than on the SDO problems.

These results indicate that intercurrent testing on two kinds of complex discriminations does not produce appreciable intertask interference in sophisticated monkeys, and that monkeys are more proficient at solving both SDO and SDP problems than was suggested by previous research.

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INTELLIGENCE AS RELATED TO GROUPING IN LEARNING*

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A. INTRODUCTION

According to Woodworth (9, p. 28), Ebbinghaus observed that it was impossible for him to avoid some grouping and accenting of the syllables he was learning, and Mueller and Schumann soon pointed out that grouping the material in rhythmical patterns may well facilitate the learning process. A very similar point has been emphasized in recent years by Katona (7). None of these researchers, however, related differential grouping to differential intelligence.

From a "common sense" point of view, one might suspect that an intelligent individual would, in comparison with a less intelligent person, easily discover the presumably-helpful principle of grouping and use it extensively. Using a formal theory rather than "common sense," Honkavaara (5, 6) has published treatises which clearly imply that intelligence is correlated with ability to organize ungrouped material into distinct groups. Experiments by Krech and Calvin (8) and Calvin *et al.* (2) indicate that the ability to reach a high level of "perceptual organization" correlates significantly with intelligence. However, Krech and Calvin measured "perceptual organization" in terms of the reproduction of stimulus material rather than in terms of the reorganization of stimulus material.

The present paper reports an experiment designed to provide objective evidence regarding the hypothesis that relatively intelligent persons have a relatively pronounced tendency to group (in Gestalt terminology, to impose structure on) material to be learned.

B. METHOD

1. *Subjects*

The 49 *S*s included 31 men and 18 women from an introductory psychology class. They were volunteers, but the instructor offered nominal credit for participation in the experiment.

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2. *Equipment*

One tape recorder was used to present the material to be learned by the *S*s, as well as part of the instructions. The material was composed of sets of randomly-selected digits. The number of digits in each set ranged from four through 10 inclusive. A second tape recorder was used to record the *S*s' responses.

Also used was an abbreviated form of the Wechsler Adult Intelligence Scale. According to Himmelstein (3, 4), the correlation between the abbreviated form and the complete test is .903.

3. *Procedure*

Each *S* was administered the abbreviated intelligence test before being introduced to the next phase of the experiment, which utilized the tape on which the digits had been recorded. The tape included these instructions: "You are going to hear some numbers. Listen carefully, and when I am through, say them after me." If *S* did not repeat the digits correctly on his first trial, they were repeated. All *S*s were required to learn all sets of digits to a criterion of one errorless repetition. The instructions were summarized each time a new set of digits was introduced.

*S*s' recorded responses were judged by three college professors who did not know the purpose of the experiments. The task of the judges was to decide whether or not each *S* grouped the digits. The number of judges who said *S* grouped was designated as that *S*'s "grouping score."

After *S* had completed the last memorization trial, he was asked to introspect regarding the method he used in memorizing the digits.

C. RESULTS AND DISCUSSION

The hypothesis that relatively intelligent *S*s have a relatively great tendency to group material to be learned was tested by computing the correlation between intelligence scores and grouping scores. The correlation was only .06, which does not differ significantly from zero.

As a second test of the hypothesis, *IQ* scores of *S*s who said they grouped the digits were compared with the scores of *S*s who did not. (The statements regarding grouping were made after the *S*s' introspection regarding their methods of memorization.) The mean *IQ* of *S*s who said they did group was slightly higher, but the difference was not significant at the .05 level of confidence ($t = 1.56$). Thus, classification of *S*s on the basis of introspective reports also yields negative results.

In spite of the negative results, the experiment seems to suggest two further possibilities. One concerns the range of talent employed; in the present study, the range of intelligence scores was from 86 to 138, with mean and standard deviation of approximately 113 and 15 respectively. It is possible that the experimental hypothesis might be verified with a population including many *S*s of low intelligence and with an abnormally high standard deviation.

A second possibility for future research concerns the frequently-reported learning plateau, as exemplified in the well-known experiment by Bryan and Harter (1). Incidental observations by the present writers suggest that grouping may be most important when a plateau is reached, and that experimentation might profitably be aimed at this phenomenon.

D. Summary

In a test of the hypothesis that individuals of relatively high intelligence have a relatively great tendency to group (or impose structure on) material which they are to learn, 49 college students were required to learn sets of digits. A measure of each *S*'s tendency to group the digits was obtained, and these measures were correlated with intelligence scores. The resulting correlation was only .06, and did not support the hypothesis.

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EFFECT OF ADRENAL DEMEDULLATION, OPERATIVE STRESS, AND NOISE STRESS ON EMOTIONAL ELIMINATION*

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A. Problem

In a previous investigation on the relationship between adrenalin and fear Moyer (6) showed that adrenalectomized rats show significantly more emotional elimination than do controls. The following was offered as a possible explanation of these results. Secher's (10) experiments indicate that the adrenal cortex is more essential to sympathetic nerve transmission than the adrenal medulla. The work of Darrow and Gellhorn (2) supported by experiments by Bulbring and Burn (1) suggests that adrenalin has a depressing effect on autonomic nerve excitability. With these results in mind, it was suggested that a total bilateral adrenalectomy reduced both the facilitating and the inhibitory effects of the adrenals on the autonomic nervous system. If the inhibiting effect is of greater consequence than the facilitating, the obtained results might be expected. The open field, because of its strangeness to the rat, is a fear producing stimulus which triggers sympathetic activity of sufficient strength to initiate compensatory parasympathetic discharges of sufficient intensity to cause sphincter dilation and consequent emotional elimination. A part of the sympathetic activity is presumably the activation of the adrenals which both facilitate and depress the A.N.S. If the inhibitory effect is greater than the facilitative, it would be expected that total adrenalectomy would result in greater emotional elimination.

A logical test of this hypothesis is to remove the medulla of the adrenal leaving the cortex relatively intact. It would then be expected that the adrenal demedullated rats would show significantly more emotional elimination than controls.

It was the purpose of this experiment to test this hypothesis. Further, the opportunity presented itself for testing the effects of noise stress on the emotional elimination of adrenal demedullated, operated control, and control rats.

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B. Method

1. *Subjects*

In this study 60 male hooded rats from Carnegie Institute of Technology colony were used. All *S*s were between 95 and 100 days of age at the time of the operation and between 110 and 115 days of age at the time of testing.

2. *Apparatus*

The open field apparatus consisted of a table top covered with white oil cloth. It was 23 x 33 in. with sides of composition board painted gloss white 15 in. high. A 200-watt bulb was placed 23 in. above the table, and the table top was 29 in. above the floor.

3. *Procedure*

Two experiments using 30 *S*s each were done with exactly the same procedure except for the noise variable which will be covered later. Fifteen to 20 days prior to the testing period 20 of the animals were operated on and a bilateral adrenal-demedullation was performed following the technique described in Farris and Griffith (3). Ether was used as an anesthetic. A sham operation was performed on 20 *S*s. The procedure for this operation was exactly the same as for the adrenal-demedullation. An incision was made in the skin and in the muscle tissue so that the adrenal glands were exposed. They were not, however, disturbed. All *S*s were then given a recovery period of 15 to 20 days before the test trials were begun. During this time the non-operated control group of 20 *S*s remained undisturbed in their home cages. Food and water was available ad lib to all *S*s throughout the experiment.

At the end of the postoperative period, all *S*s were given the open field test. Each *S* was placed in the center of the open field for a 4-min. period once a day for seven days. A record was kept of the number of fecal bolli produced, and whether or not the *S* urinated on any given day. Between trials, the sides were removed and the open field was washed with warm water, then allowed to dry. During the seven days on which the *S*s were placed in the open field, a daily 24-hr. bollus count was made in the home cage.

During the postoperative recovery period of the first experiment, construction changes were being made in the laboratory that involved a considerable amount of banging on pipe and general noise making, a radical change from the usual quiet maintained in the rat room. This replication consisting of 10 *S*s in each group is designated as the noise stress replication.

The second experiment was carried out using exactly the same procedure as the first except that all alterations were finished and the usual quiet returned to the laboratory.

C. RESULTS

Three scores of emotional elimination were recorded: (*a*) the number of day on which an *S* defecated; (*b*) the number of days on which an *S* urinated, and (*c*) a weighted emotional bollus count which was computed as the ratio of the average number of emotional bolli a *S* produced each day in the open field to his average home cage daily bollus count. This weighting was used as a control since an animal which had a higher daily bollus production might be expected to produce more bollus in the open field situation than an animal with a low daily bollus production.

The data for the Stress and Non-stress studies were combined to form a factorial design (fixed constants model, see McNemar, 4, p. 306) in which the Stress condition was represented by the rows and the Operation condition by the columns.

A Bartlett's test for homogeneity of variance indicated that no significant heterogeneity of variance existed. A Chi-Square of 11.07 is required for significance at the .05 level. Chi-Square for days defecated was 4.13; for days urinated 8.75, and for weighted emotional bolli 9.86.

The .05 level of confidence was selected for significance and analyses of variance performed on the data for each of the three dependent variables. These analyses are summarized in Table 1.

Turning first to the analysis of the days defecated data, the significant interaction becomes the primary concern. It is apparent that the relationships between days defecated and operation condition are different for the Stress and Non-stress conditions. The left-hand graph in Figure 1 illustrates this interaction. The analysis of the simple effects given in Table 2 indicates that both the Demedullated-Stress and the Operated Control-Stress groups, while not significantly different from each other, do differ significantly from the Control-Stress and all three of the Non-stress groups. The difference between the Control-Stress and Control Non-stress groups is very small and not significant.

The interactions from the analyses of the days urinated and the weighted emotional bollus scores are not significant, although these curves, as shown in Figure 1, do bear considerable resemblance to those for days defecated. In these analyses, the main effects are significant. However, because a significant interaction had appeared for days defecated and because of the similarity in

TABLE 1
Analyses of Variance on Three Emotional Elimination Scores for Two Stress and Three Operation Conditions

Emotional elimination variable	Source	Sum of squares	df	Mean^2 Est.	Error term	F
Analysis 1						
	1 Stress	52.27	1	52.27	4	18.024*
Days defecated	2 Operation	58.63	2	29.31	4	10.107*
	3 S x O	25.43	2	12.72	4	4.386*
	4 Within	156.40	54	2.90	—	
Analysis 2						
	1 Stress	3.75	1	3.75	4	8.721*
Days urinated	2 Operation	4.43	2	2.22	4	5.163*
	3 S x O	.70	2	.35	4	.814
	4 Within	23.30	54	.43	—	
Analysis 3						
	1 Stress	.010534	1	.010534	4	13.552*
Weighted emotional bolli	2 Operation	.006057	2	.003028	4	3.896*
	3 S x O	.004177	2	.002089	4	2.687
	4 Within	.041973	54	.000777	—	

the patterns of cell means in these two analyses to the pattern of cell means for the days defecated analysis, the simple effects for days urinated and for weighted emotional bollus count were also examined.

Inspection of Table 2 indicated that the same differences between cell means that were significant for days defecated are also significant for weighted emotional bollus count. As was the case with days defecated, the existence of the stress condition did not create significant differences between the two non-operated control groups.

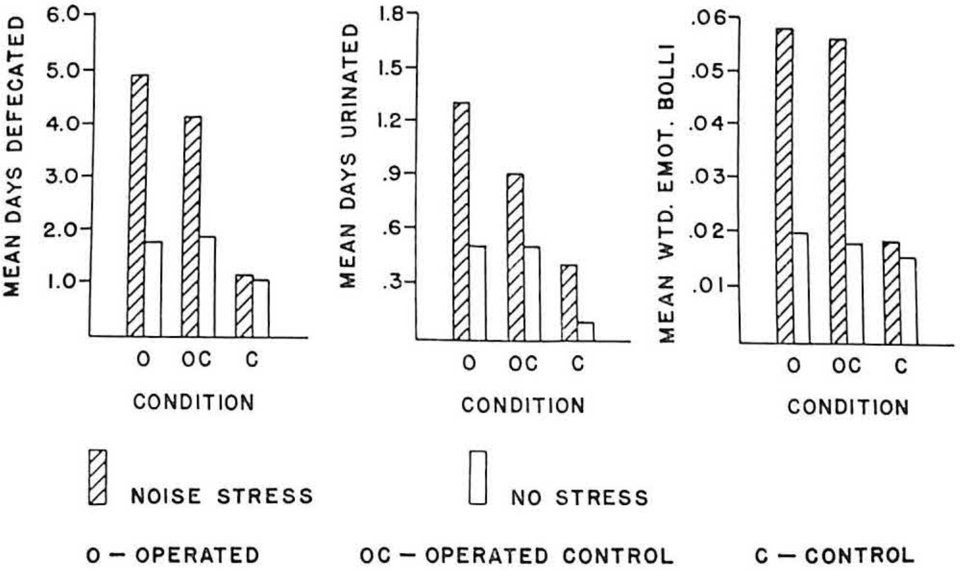

FIGURE 1

In the case of days urinated, the similarity between the analyses with regard to significant simple effects holds true for the demedullated-stress group but not for the operated control-stress group. The difference in means between the OC-S group and the D-S group is relatively larger for days urinated than for the other two dependent variables.

D. Discussion

These results do not appear to support the hypothesis that adrenal-demedullated rats will show more emotional elimination in the open field than controls. The results from the second experiment where no extraneous noise stressor was introduced indicate that there are no significant differences between the three groups. Adrenal demedullation then, which removes essentially all of the extra-sympathetic chromaffin tissue (11), has no effect on emotional behavior as measured by the Hall open field test. Although these data do not support the explanation given by Moyer of the results of the adrenalectomy study (6), they do lend further support to the idea that in the rat, at least, adrenalin is not an important factor in the physiology of the fear syndrome. These data are clearly consistent with previous research from this laboratory. It has already been shown that adrenalectomy does not effect the formation or extinction of an escape response (5); adrenal demedullation does not effect the learning of an avoidance response (9); and injected adrenalin does not effect either a startle response or the learning of an avoidance response (7, 8). If we look at the two experiments together, it is apparent that although adrenal demedullation per se has no effect on emotional elimination, stress significantly increases it. The two operated groups in the non-stress experiment differ significantly from the two operated groups in the stress experiment, but they do not differ from each other. Also, the two operated groups in the stress experiment differ significantly from the control group but the means of the control groups in the two experiments are very

TABLE 2
MEANS AND DIFFERENCES BETWEEN MEANS ON THREE EMOTIONAL ELIMINATION SCORES FOR TWO STRESS AND THREE OPERATION CONDITIONS

Condition	Mean		OC-S	C-S	D-NS	OC-NS	C-NS
A. Days Defecated: Critical Difference for .05 level of Confidence = 1.52							
Demedullated-stress	4.9	D-S	.80	3.80*	3.20*	3.10*	3.90*
Operated control-stress	4.1	OC-S		3.00*	2.40*	2.30*	3.10*
Control-stress	1.1	C-S			.60	.70	.10
Demedullated-no stress	1.7	D-NS				.10	.70
Operated control-no stress	1.8	OC-NS					.80
Control-no stress	1.0						
B. Days Urinated: Critical Difference for .05 level of Confidence = .55							
Demedullated-stress	1.30	D-S	.40	.90*	.80*	.80*	1.20*
Operated control-stress	.90	OC-S		.50	.40	.40	.80*
Control-stress	.40	C-S			.10	.10	.30
Demedullated-no stress	.50	D-NS				.00	.40
Operated control-no stress	.50	OC-NS					.40
Control-no stress	.10						
C. Weighted Emotional Bolli: Critical Difference for .05 level of Confidence = .0244							
Demedullated-stress	.0580	D-S	.0019	.0399*	.0383*	.0402*	.0428*
Operated control-stress	.0561	OC-S		.0380*	.0364*	.0383*	.0409*
Control-stress	.0181	C-S			.0016	.0003	.0029
Demedullated-no stress	.0197	D-NS				.0019	.0045
Operated control-no stress	.0178	OC-NS					.0026
Control-no stress	.0152						

nearly the same. This must mean that neither operative trauma nor noise in and of itself is sufficiently stressful to affect the measured response. It is only when the two are combined that the *S* is sufficiently stressed to react with increased emotionality.

It appears then that an intense stressor increases emotional elimination, and that the functioning of the adrenal medulla is not a factor in this reaction of the rat to stress.

E. SUMMARY

Sixty male hooded rats were used in this study. Twenty animals were subjected to a bilateral adrenal-demedullation. Twenty were given the same operation except that the adrenals were not disturbed, and 20 served as non-operated controls. Each of these three groups were then divided into two groups of 10 each for a stress experiment and a non-stress experiment. The *S*s in the stress experiment were subjected to noise during the postoperative period. The postoperative period for the non-stress groups was relatively quiet. At the end of the postoperative period, all animals were placed individually in an open field situation for four minutes a day for seven days and the amount of emotional elimination was recorded.

The results show that emotional elimination is not effected by adrenal-demedullation, operative trauma, or noise. However, operative trauma and noise combined produces a significant increase in emotional elimination whether the adrenal medulla is present or not.

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